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ART. LIV.—*On a possible cause of the Variations observed in the amount of Oxygen in the Air;* by EDWARD W. MORLEY, M.D., Ph.D., Hurlbut Professor of Chemistry, Western Reserve College, Hudson, Ohio.

IN order to determine, if possible, whether the observed variations in the proportion of oxygen contained in the atmosphere at different times is occasioned by the descent of air from an elevation above the surface of the earth, I have made two series of analyses in duplicate of samples taken each day at this place. The results are given in the accompanying table.

The results for the first period have been compared graphically with the indications of the thermometer and barometer furnished me by the Signal Service observer at Cleveland. At times, when the meteorological conditions of the region were simple enough, it was easy to see that the deficiencies in the proportion of oxygen followed closely times of high barometer and low temperature, when, if ever, it would be fair to infer that the descent of air from an elevation would take place. But in a great number of cases it is impossible to infer very much as to the atmospheric currents of the region from observations at one place.

But if we examine the thrice-daily maps of the state of the thermometer, barometer and winds, as observed by the Signal Service, we may obtain reasonably good evidence as to the atmospheric currents of this region at the times of deficiency of oxygen observed. When we find this place at or near a center of high pressure, and find the recorded directions of the

Oxygen found in Air at Hudson, Ohio, from January to December, 1880.

January, 1880.		February.		March.		April.		May.		June.		October.		November.		December.	
Date.	Oxygen.	Date.	Oxygen.	Date.	Oxygen.	Date.	Oxygen.	Date.	Oxygen.	Date.	Oxygen.	Date.	Oxygen.	Date.	Oxygen.	Date.	Oxygen.
1	956 969	1	949 945	1	935 943	1	968 961	1	952 942	1	962 961	1	947 951	1	969 979	1	967 967
1[1]	962 955	1[9]	954 950	2	932 943		968	2	930 935	2	961 948	2	945 949	2	951 949	2	963 966
2	959 957	2	953 954	3	944 929	2	959 949	3	956 967	3	958 957	3	961 960	3	969 962	3	969 975
3	976	3[7]	947 945	4	931 938	3	964 960	4	948 949	4	946 955	4	954 951	4	968 963	4	961 958
4	959 954	4	946 944	5	947 951	4	959 969	5	956 962	5	953 952	5	949 955	5	984 969	5	962 963
5[1]	947 948	5	949 951	6	941 943	5	959 966	6	963	6	944 949	6	958 967	6	967 976	6	956 959
6[3]	958 968	6	948 950	7	944 942	6	958	7	955 954	7	948 940	7	946 958	7	951 952	7	964 959
7[2]	955 958	7	959 958	8	962	7	949 951	8	977	8	955 952	8	963 958	8	954 969	8	946 954
8[4]	941 936	8	959 951	9	001 006	8	972 963	9	946 942	9	953 941	9	958	9	976 963	9	955 965
9[2]	936	9	910 917	10	946 954		963	10	953 947	10	958 962	10	951 954	10	960 973	10	972 961
10[4]	947 944	10	931 927	11	911	9[2]	941 940	11	948 958	11	958 962	11	961 960	11	956 965	11	959 957
11[4]	960 965	11	940 944	11[8]	923 918	10	984 985	12	952 951	12	959 962	12	949 961	12	964 963	12	967 967
12[3]	963	12	940 927	12	925 932	11	976 972	13	949 954	13	949 952	13	957 959	13	964 954	13	970 967
13[4]	955 965	13	899 896	13	949 956	12	971 969	14	950 952	14	952 951	14	963 966	14	965	14	958 959
14[3]	967 952	14	867	14	935 929	13	965	15	953 944	15	942 947	15	956 960	15	957 958	15	948 944
15[4]	960	14[8]	893 906	15	926 925	14	942 947	16	927 928	16	958	16	970 964	16	966 974	16	957 959
16[2]	954	15	901 915	16	918 917	15	976	17	lost	17	935 951	17	956 970	17	964 975	17	954 961
17[3]	969	16	927 920	17	948 950	16	950 954	18	948 937	18	935 930	18	972 963	18	951 957	18	960 950
18[4]	939	17	944 940	18	961 958	17	960 963	19	950 952	19	950 949		970	19	961 955	19	944 943
19[2]	962	18	946 941	19	951 961	18	967 962	20	945 947	20	948 961	19	956 969	20	963 965	20	953 967
20[4]	954 947	19	958 958	20	958 953	19	965 964	21	928 933	21	955 959	20	961 974	21	959 957	21	956 956
21[3]	928 933		956	21	968 975	20	960 960	22	934 926	22	945 940	21	954 969	22	962 964	22	962 956
22[4]	948 949	20	925 934	22	961 956	21	959 970	23	939 952	23	958 960	22	961 948	23	947 955	23	951 954
23	943 946	20[8]	926 924	23	958 963	22	959 965	24	953 956	24	955 947	23	962 967	24	862 967	24	951 952
24	947 954	21	932 936	24	941 943	23	963 961	25	955 966	25	948 962	24	962 972	25	965 951	25	945 945
25	952 956		926	25	954 967	24	958 967	26	920 917	26	957 947	25	948 952	26	964 963	26	955 955
26	931	22	935 934	26	972 977	25	964 965		920	27	958 956	26	967 956	27	960 955	27	967 954
27	941 947	22[8]	913	27	961 968	26	963 965	27	959 961		956	27	866 868	28	974 968	28	952 957
28	951	23	954 948	28	963 964	27	970 968	28	969 953	28	911 908		869 874	29	967 965	28[8]	948 945
29[5]	922 931	24[1]	946 948	29	958 965	28	963 950	29	958 960	29	952 947	28	975 971	30	964 956	29	959 958
30	939	25	946 944	30	922 921	29	943 940	30	960 970	30	952 963		980 976			29[8]	957 956
31	965 959	26	920 924	31	962 961	30	939 947	31	961 957			29	981 969			30	960 955
		26[8]	902 910									30	930 938			30[8]	958 951
		27	940 950									31	952 947			31	955 947
		28	945 950													31[8]	956 947
		29	938 935														

Oxygen found in the Air at Hudson, Ohio, from January to April, 1881.

January, 1881.		February.		March.		April.	
Date.	Oxygen.	Date.	Oxygen.	Date.	Oxygen.	Date.	Oxygen.
1	949 939	1	958 961	1	948 953	1	972 972
1[8]	936 940	2	959 953	2	950 956	2	974 975
2	954 960	3	954 960	3	967	3	965 964
2[8]	952 947	4	952 962	4	961 962	4	967 968
3	959 957	5	964 959	5	962 969	5	970 973
4	954 951	6	949 962	6	958 962	6	972 974
5	958 958	7	963 963	7	969 969	7	973 968
6	956 953		966 966	8	964 962	8	962 972
7	958 948	8	968 965	9	965 963	9	962 966
8	946 960	9	959 956	10	955 962	10	967 971
9	954 962	10	951 950	11	967 966	11	963 971
10	966 967	11	950 946	12	948 959	12	966 967
11	957 959	12	963 961	13	958 958	13	963 969
12	963 956	13	953 951	14	962 964	14	973 971
13	958 957	14	948 957	15	962 965	15	975 970
14	957 961	15	958 962	16	969 964	16	968 970
15	960 961	16	951	17	960 963	17	970 966
16	952 952	17	961 967	18	972 965	18	969
17	959 957	18	955 962	19[12]	960 960	19	964 960
18	950 952	19	959 961	20	956 954	20	969 963
19	956 956	20	957 954	21	974 974		
20	958 960	21	961 968	22	957 957		
21	948 950	22	950 954	23	970 970		
22	938 933	23	968 962	24	970 967		
23	955 947	24	963 972	25	967 969		
24	959 958	25	959 961	26	960 962		
25	952 947	26	958 959	27	962 963		
26	969 967	27	958	28	976 977		
27	959 959	28	943 950	29	952 955		
28	953 961			30	957 959		
29	960 954			31	973 972		
30	968 964						
31	960 962						

The figures in the column "Oxygen" are the 3d, 4th and 5th decimal places: before which are to be supplied the decimal point, followed by the figures 20. For example, 956 signifies 0·20956. But on March 9, 1880, supply 0·21. [12], collected at noon; [1], [2], [3], [4], [5], [8], [9], collected at 1, 2, 3, 4, 5, 8, 9, P. M.; [s], second sample collected nearly at the same time as the preceding sample.

winds at the stations of the Service all radiating from this center, especially if their velocities are considerable, we may infer with some fair probability that within this area of radiation there was a descent of air from an elevation.

I have, therefore, examined the Signal Service maps of the date of each deficiency in oxygen, or of each noteworthy fall in the amount of oxygen. The reproducing a score of these maps here would facilitate the forming an opinion as to the soundness of my hypothesis; but since the morning maps are easily accessible to most of those who would be interested in this

paper, and at no very long time after their date, I think it sufficient to describe what I learn from the examination, and to refer to the maps themselves those who may desire to form an independent opinion. Meanwhile I give, necessarily somewhat in detail, an account of the indications gained from my own comparison of the maps with the results of the analyses.

The analyses here tabulated have been made with the improved apparatus alluded to in a former article in this Journal as intended to lessen the probable error of an analysis. A comparison of the duplicate analyses made on the same sample will show that this intention has been carried out.

Jan. 9, 1880, 7 A. M.—There was an area of low pressure over the western part of Illinois, and of high pressure over the Gulf of St. Lawrence. The isobar of 30·10 inches ran from Montreal along the Appalachians to the Gulf of Mexico. East of the Appalachians, from Albany to near Knoxville, there were gentle winds of three or four miles an hour. They were not so directed as to indicate any general current across the mountains. West of the mountains there were brisk winds of eight miles an hour at Columbus, Cincinnati and Indianapolis, of nine miles at Pittsburgh, of ten miles at Buffalo and Louisville, and of from fifteen to twenty miles at Cleveland, Toledo and Erie. All these were nearly transverse to the axis of the Appalachians. A study of the weather map of this date suggests as a probable view that the mass of air passing to the northwest over Ohio was not part of a current passing at the surface of the earth over the Appalachians, but that it was in part the result of a descent of some upper current to the surface of the earth. Similar conditions continued at 3 P. M. At 2 P. M. the oxygen in the atmosphere at this place was 0·20936. It had been low on the preceding day, but observations of the wind and barometer were insufficient to determine the direction of the great currents of the air.

Jan. 10, 7 A. M.—There was an area of high barometer having its center over southwestern Ohio. From a little to the northwest of this center, winds were distinctly radiating in every direction with a velocity of about five miles an hour. It is therefore probable that the central area of high barometer was kept supplied by some current of air from an elevation. At 3 P. M. the center of radiating winds had moved so as to be over Lake Ontario. Cleveland was on the curve of 30·30 inches pressure. No winds were entering this curve at the surface of the earth as far as we can learn from the weather map of this hour, while they were moving outward all around it with a mean velocity of several miles an hour. The high pressure was not materially decreased by this outflow, and this continuance of high pressure was not due to rise of tempera-

ture for the region had become colder. It is, therefore, highly probable that some current of air from an elevation entered the area; and at 4 P. M. the oxygen found here was 0·20945.

Jan. 18, 3 P. M.—From the directions of the winds on the map of this date we could not be sure that the currents of air passing over Ohio had not come from the Gulf of Mexico after passing around a center of high pressure. But the maps for forty hours previously make it reasonably sure that no currents at the surface of the earth had brought air from the Gulf of Mexico to the vicinity of Cleveland, and so lead us to infer that the sample collected here at 4 P. M. was from air spreading out in all directions from the center of high pressure just mentioned, which was over the States of Mississippi and Tennessee. It contained 0·20939 oxygen.

The deficiency of oxygen found on January 21st is consistent with the theory proposed, but affords no evidence.

Jan. 26, 7 A. M.—There was an area of high barometer over the seaboard from Virginia to Maine. Over the southern part of Pennsylvania there was an obvious center of winds radiating in all directions with a mean velocity of about five miles an hour. It is probable that the withdrawal of air from this area was made good by the entrance into it of air from some upper current. At 9 A. M. the amount of oxygen in the air at this place was 0·20931.

Jan. 29, 3 P. M.—At this time there was an area of high barometer having its center a little to the northwest of Montreal. The same point seems also to have been a center from which winds radiated in every direction. Although there are no stations reporting from the northern half of the supposed circle, a circular course of isobars is clearly indicated; the winds to the east of the center blow to the east, and those to the west of the center blow due west. The mean velocity of winds passing out from the southern half of the assumed circle is ten or twelve miles an hour. If, then, there was a closed curve to the north, where the air was also passing out, there must have been a rather rapid supply of air brought into this area by currents from an elevation, and at 9 A. M. the oxygen found at this place was 0·20926.

Feb. 9, 7 A. M.—There was at this time an area of high barometer within which the isobar of 30·40 inches was nearly a circle having Lake Michigan for its center. Stations are but few to the north and northwest of this circle, so that it cannot be absolutely affirmed that winds were blowing outward all around the circumference. But the gentleness of the winds over the northern half of the circle, and the briskness of winds passing over its southern semi-circumference, point to a descent of upper currents. The oxygen found here at 9 A. M. was 0·20914.

Feb. 10, 7 A. M.—High pressure now prevailed off the coast of New England. The form of the isobars and the directions of the winds reported show a gentle current of air along the eastern slope of the Appalachians, but no motion across them. The crowding together of isobars running along the axis of the mountains, as well as the uniform northwest motion of the winds over an area reaching from Cincinnati to Oswego and Kingston, make it probable that air from an upper current came down to the surface of the earth near lake Erie; and the oxygen found here at 9 A. M. was 0·20929.

Feb. 11, 7 A. M.—At this hour there was an area of high barometer over Chesapeake Bay. The winds were all moving away from the Appalachians, both on the eastern and the western slopes. It is therefore likely that there was a descent of air from some upper current. The oxygen found here at 9 A. M. was 0·20942.

Feb. 12, 7 A. M.—There was now an area of high barometer off the coast of South Carolina. East of the Appalachians most of the winds were parallel to the axis of the chain. West of the mountains the winds may well enough have come from the Gulf of Mexico, but the acceleration of the winds as we look toward the center of low pressure near the upper lakes may perhaps indicate a descent of upper currents. The oxygen found at 9 A. M. was 0·20934.

On Feb. 13th the oxygen found was 0·20897; this case needs special discussion.

Feb. 20, at 7 A. M.—There was an area of high pressure having its center near Pittsburgh. Winds radiated in all directions from this center. The inference of the descent of upper currents is probable. At 9 A. M. the amount of oxygen found here was 0·20930, and in another sample taken at nearly the same time, 0·20925.

At the two hours of observation next following the one just mentioned, the same divergence of winds from a center seems to have continued, the center passing slowly to the east. Now at 7 A. M. on the 21st, no such condition of radiating winds continued, but the oxygen found at 9 A. M. was 0·20926. Possibly the long continuance of conditions favoring the descent of upper currents had brought to the surface of the earth near this place a volume of air poor in oxygen which had not yet been carried away under succeeding conditions.

February 22d.—At 7 A. M. there was an area of high barometer a little to the south of the Ohio River. At 9 A. M. the oxygen found here was 0·20934, and in another sample 0·20913.

A deficiency of oxygen was also observed on February 26th, and on March 11th, 12th and 15th, but I am able to suggest no satisfactory explanation of either case.

March 24th.—At 7 A. M. there was an area of high pressure having its center at Lake Superior. Winds were diverging all around the southern half of a circle; no stations were far enough to the north to give any certain knowledge as to their direction over the northern half. The oxygen found here at 9 A. M. was 0·20942.

The same remark may be made as to March 30th. A center of high pressure existed near Lake Superior, and the winds radiated from the southern half of a circle. The oxygen found at 9 A. M. was 0·20922.

April 9th.—At 7 A. M. there was an area of high barometer reaching from Texas to Tennessee. North of latitude 37 degrees, and east of St. Louis, almost every reported wind was blowing toward the north or northeast, while on the south of the same line the winds reported were all blowing toward the south or southwest. We may therefore suppose that some descent of upper currents would occur; at 9 A. M. the oxygen in the air here was 0·20940.

April 14th.—At 7 A. M. there was a center of high pressure off the coast of Georgia, but the data are too incomplete for trustworthy inference. The oxygen found at 9 A. M. was 0·20945. On the 16th, the data are also too incomplete.

April 28th.—At 1 A. M. there was an area of high pressure with distinctly radiating winds having their center on the Ohio River. At 7 A. M. this center was over West Virginia. At 9 A. M. the oxygen found in the air here was 0·20957, which was a fall of 0·00012. The cause continued to operate for some time afterwards, and on the next day the oxygen found was 0·20941. On the 30th, in spite of the passage of an area of low barometer the oxygen continued low, being found to be 0·20943. On May 1st there was a center of radiating winds over the southeast part of Kentucky. Under the influence of the descent of upper air which probably occurred, the oxygen found at 9 A. M. was 0·20947.

On the 2d this center of radiating winds was over North Carolina, and the oxygen found here was 0·20932 at 9 A. M.

On the 9th and 10th an area of high barometer hovered over the sea coast of North Carolina for twenty-four hours, with well marked diverging winds over the observed half of the circumference of a circle. On the 9th the oxygen found here was 0·20944, and on the 10th it was 0·20950, both at 9 A. M. An area of high pressure now developed near Lake Michigan and hovered to the north of the lower lakes till the morning of the 16th, when it passed over the lakes toward South Carolina. During these days the oxygen found here at 9 A. M. was 0·20950, 0·20953, 0·20951, 0·20951, and 0·20948, and on the 16th when the area of high pressure became central near Cleveland, the oxygen found at 9 A. M. was 0.20927.

I cannot suggest an explanation of the deficiency in oxygen which occurred May 21st and 22d.

May 26th.—At 7 A. M. there was an area of high pressure over Georgia, with winds spreading in all directions around the landward side of a circle. At 9 A. M. the oxygen found was 0·20919.

June 17th.—At 7 A. M. there was a large area of slight excess of pressure covering most of the Northwestern States. On the 18th at 7 A. M. the high pressure had moved to the east, and the highest pressure observed was at Cleveland. Under its influence, the amount of oxygen found at 9 A. M. was 0·20933.

The deficiency of oxygen observed on June 28th I am not able to explain; the data being insufficient.

Having made my application too late, I have not obtained a series of the thrice-daily weather maps for comparison with my observations on variations in amount of oxygen for a period of six and two-thirds months beginning with October, 1880. A comparison of my observations with the daily morning maps leaves some facts unexplained which the possession of fuller data might clear up satisfactorily. All the maps used being of the seven o'clock series, it will not be needful to specify the hour further; the observations of oxygen were all made at the same time with the observations from which the maps were made.

I cannot explain the deficiency of oxygen occurring on October 4th.

On October 5th there was a long narrow area of high pressure reaching from New England to Texas. The spreading out of the air on each side of this area was well marked, the inference that there was a descent of upper currents to the surface is well sustained, and the oxygen found was 0·20952.

October 7th. An area of high pressure had its center near Lake Erie, with winds spreading outward in all directions. The oxygen found was 0·20952, a fall of 0·00010.

October 10th.—A long narrow area of high pressure extended from Maine to Texas. The radiation of winds in all directions was decided, and the inference that downward currents mingled the surface air with air from an elevation reasonably probable. The oxygen found here was 0·20953. But this was a fall of only 0·00007, which is a pretty small difference. The probability that there really was a fall in the amount of oxygen is only about six to one.

The deficiency of oxygen on October 22d I cannot explain.

On the 25th, winds were radiating in all directions from an area of high pressure in Eastern Tennessee. The oxygen found was 0·20950, a fall of 0·00017 from the previous day.

The chances that there was really a fall are here about 8000 to 1.

October 27.—There was an area of high barometer with its center well to the north near Lake Superior. Over the observed half circumference of a circle, the winds were distinctly spreading in all directions. The inference that there were downward currents of air is also supported by the fact of an apparent excess of velocity in the winds passing south over the lakes over the velocity of winds approaching them from the north. Under the influence of this or some other cause, the oxygen found here was 0·20869.

I cannot explain the deficiency of oxygen on October 30th.

On November 2d there was an area of high pressure with its center over Tennessee and North Carolina. In a general way, winds diverged from Kentucky and West Virginia. But there were counter currents, so that with the materials at hand we are not authorized to say that a descent of upper currents is indicated with any such probability as to explain the deficiency of oxygen found. The amount was 0·20950.

November 7th.—There was an area of high pressure with its center over Mississippi. From this winds diverged all around the northern half of a circle. There were some local winds in Upper Lake Region which did not conform to this system; but the shape of the isobars makes it fairly probable that in the region of the lower lakes there were descending currents of air from the upper part of the atmosphere. The oxygen found was 0·20951, a fall of 0·00020 since the day before.

November 18th.—There was an area of high pressure over the Indian Territory. From this area winds were spreading outward over the observed half circle of stations. An acceleration of the winds in the Lower Lake Regions suggests a supply of air from above to feed these winds. On the 19th this area of high pressure had its center in Southern Ohio. The inference of a descent of upper currents is as clear as it can be from maps with no more reporting points than those now established. The oxygen found was 0·20958, a fall of 0·00010 since the 17th. On the 18th it had been 0·20954.

On November 23d there was an area of high pressure with its center over Southern Ohio, and the inference that upper currents reinforced the winds which spread out all around this area is a probable one. The oxygen found was 0·20951, a fall of 0·00012 from the preceding day.

December 3.—At this time there was an area of high pressure with its center over Southern Ohio. It had moved with great velocity from over the Upper Missouri Region; winds were diverging from a center over Southwestern Ohio. The oxygen found was not affected. On the 4th the area of high

barometer was central over Chesapeake Bay, and the winds blowing toward the northwest from the Appalachian Mountains seemed not to blow over them from the east. The oxygen now found was 0·20960, which was a fall of 0·00012 from the preceding day.

The deficiency of oxygen on the 8th I cannot explain satisfactorily.

On December 14th there was a high barometer off the coast of Florida. As often happens, the Appalachians seemed to act as a barrier. The winds blowing from them toward the northwest had nothing to do with the winds to the east of the mountains. If we may thence infer a descent of upper currents in this region, we shall account for the fall in the amount of oxygen, which amounted to 0·00010.

The deficiency in oxygen noticed the next day is easily explained according to the working hypothesis suggested, but does not add to the evidence for it. The same is true of the deficiency observed on the 19th.

On December 23d, a very wide area of high pressure with winds diverging from the area affords a reasonable presumption that these winds were reinforced by a descent of upper currents. The oxygen found was 0·20953, a fall of 0·00006, which is too small for safe deduction. On the 24th and 25th the center of high pressure was nearly stationary over the lower St. Lawrence. The directions of the winds are rather confused, but distinctly exhibit the tendency according to which winds radiating from a common center of high pressure are likely to be accompanied by a deficiency of oxygen. The oxygen found was 0·20951 and 0·20945 respectively on the two days.

The rapid fall in the amount of oxygen on the 28th affords no evidence for or against the theory.

The deficiency of oxygen on December 31st and January 1st was probably due to the occurrence of an area of high pressure over the Appalachian Mountains on each of those days. From each side of this area winds were blowing outward. The amount of oxygen on the 31st was 0·20951; on the 1st it was 0·20944 in the morning, and 0·20938 in the evening. From the weather map of the 2d, according to my theory it would be expected that there would be a deficiency of oxygen; but while the evening observation showed a slight deficiency, the morning observation showed none.

The map for the 22d gives reports from but a few stations, so that the data are too few for trustworthy inference. As far as the facts go, they seem little conformed to the theory.

January 25th.—At this date there was an area of high pressure over Mississippi. Winds were spreading outward in all directions around this area, the oxygen found was 0·20949.

The deficiency of oxygen noticed on the 28th was well explained by the occurrence of an area of high pressure over Missouri, with winds radiating around it. On the 29th, this area was central over eastern Tennessee, the winds well exhibited the spreading out in all directions which suggests the descent of upper currents. The oxygen found on these days was 0·20957, a fall of 0·00011 as compared with the 26th.

The morning of February 9th affords a reasonably clear proof that the surface winds implied the descent of upper currents. There was an area of low pressure over the mouth of the Mississippi, and one of high pressure on the northern half of the Atlantic seaboard. East of the Appalachians, no winds were directed across the mountains, while on the western side of the mountains, from Louisville to Montreal, the winds were all radiating from a center in Pennsylvania, with a mean velocity of eight miles an hour. On the next day the stations near Louisville were involved in currents coming from the gulf, and gentle winds were blowing from the seaboard toward the Appalachians. There was an area of low pressure in Michigan. The obvious acceleration of the winds in the lower lake region suggests a continuance of the descent of upper currents which probably occurred on the 9th. The oxygen observed on these days was 0·20958 and 0·20951, against 0·20967 on the 8th. The deficiency of oxygen continued till the 11th, with an area of high pressure reaching from eastern Pennsylvania over the lake region to the northwest. The oxygen now observed fell to 0·20948.

There was a deficiency of oxygen on the 13th. There was a general brisk motion of winds toward a center of low pressure in Maine, with nothing explaining the observed deficiency.

On the 15th, there was an area of high pressure with its center over New Jersey. Winds blew away from this center in every direction. No deficiency of oxygen was observed, however, on the morning of this day, but on the next day the oxygen found was 0·20951, a fall of 0·00009. Nothing on the maps of this morning explains this deficiency.

On February 21st, there was an area of high barometer over the lower Mississippi valley. Winds were blowing outward in all directions. The inference that there was a descent of upper currents is perhaps a fairly probable one. The oxygen was not affected at this place, being found to be 0·20964. On the 22d there was an area of low pressure over Lake Superior, toward which winds were drawn with increasing velocity from the northwest slope of the Appalachians, while on the other side of the mountains the winds show no connection with the system prevailing on the northwest side. Over the region from Louisville to Kingston the mean velocity of the winds was

eleven miles an hour. It seems almost certain that there must have been a descent of upper currents. The oxygen found on this morning was 0·20952, a fall of 0·00012.

On February 28th, the oxygen here fell to 0·20947, but there is nothing in the map of that date suggesting any explanation. On the previous day, however, there were such conditions as seem to indicate that a deficiency of oxygen was to be expected. If from other evidence my theory should seem to be a first approximation to some law of nature, it will be supposed that some air deficient in oxygen, brought to the surface of the earth by the conditions prevailing on the 27th, came to the observer here on the next day. But this is almost too precarious to be mentioned.

On March 1st and 2d, there was an area of high pressure over Lake Superior, with winds radiating around the observed third part of a circle. The oxygen found on these days was 0·20951 and 0·20953. On the 3d, there was an area of low pressure and a storm of considerable violence over the Ohio valley, and the oxygen found promptly went up to 0·20967.

On March 12th, there was an area of relatively high pressure over Lake Ontario, with a storm having its center of low pressure in Kansas. Winds were directed away from Lake Ontario in all directions, the inference that there occurred a descent of upper currents is a reasonable one, and the oxygen found was 0·20954, a fall of 0·00012.

March 19th.—At this time there was an area of high pressure in Maine, and of low pressure over western Kentucky. The barometer was three-tenths lower at Cincinnati and four-tenths lower at Louisville than it was at Cleveland, with this high barometric gradient, the winds were much accelerated in passing over Cleveland toward the southwest, and it may be that upper currents were compelled to descend and mingle with them. The oxygen found was 0·20960, a fall of 0·00008.

On March 20th, there was an area of low pressure just east of Lake Michigan. The deficiency of oxygen is not explained by the weather map. The same is also true of the 29th and 30th.

As far as I can see, it is impossible to discern any connection between the deficiencies of oxygen observed, and the direction of the wind at the time of taking the sample.

My own judgment, from the comparison detailed, is, that the theory that deficiencies in the amount of oxygen in the atmosphere are caused by the descent of air from an elevation fairly well agrees with the facts.

ART. LV.—*On Jolly's Hypothesis as to the Cause of the Variations in the Proportion of Oxygen in the Atmosphere;* by EDWARD W. MORLEY, M.D., Ph.D., Hurlbut Professor of Chemistry in Western Reserve College.

JOLLY has suggested a certain hypothesis as to the cause of those variations in the ratio of oxygen to nitrogen which are from time to time observed in the atmosphere of a given place. He supposes that the volumes of air which exhibit the deficiency of oxygen are brought by currents from the tropical regions, that the deficiency of oxygen was caused in those regions, that it was caused by the consumption of oxygen in the oxidation of organic matter, and that at some places within the tropics this consumption is therefore considerably greater than the liberation of oxygen in the processes of vegetation.

I have proposed a second hypothesis. I suppose that the volumes of air deficient in oxygen are brought by currents from an elevation above the surface of the earth, that the deficiency of oxygen was caused while these volumes were at this elevation, and that it was caused by that assumed physical action according to which, in a high vertical column of a mixture of two gases, the heavier will tend to become less abundant at the top of the column.

The labor of establishing either hypothesis by experiment will probably be considerable. I propose to mention some reasons which seem to indicate that Jolly's hypothesis is the less probable.

1. There is no direct evidence that the atmosphere near the equator is poorer in oxygen than the air of higher latitudes. Numerous analyses agree in this result. Lewy's analyses of air, collected at Guadeloupe, show that the mean ratio of oxygen to nitrogen there is the same as that at Paris.

2. It is difficult to ascribe to the cause assumed by Jolly a magnitude sufficient to produce the observed effect.

If a volume of air at latitude fifty degrees is deficient in oxygen by 0·004 or 0·005, the deficiency must have originally been far greater, if this air has come from the tropics, and has thus for many hundreds of miles been exposed to admixture with normal air. We must either, in the first place, believe that at some parts of the tropical regions there are not very seldom immense volumes of air, deficient in oxygen to the amount of 0·01 or more; or, in the second place, we must assert that the analyses which show deficiencies of oxygen at latitude fifty degrees amounting to 0·004 or 0·005 are grossly in error, and that the actual deficiencies are very much less; or, in the third place, we must abandon the hypothesis. If the analyses are

trustworthy we must abandon the hypothesis, or else attribute to its supposed cause a magnitude altogether incredible. I will examine some of the experimental evidence, that the oxygen in the atmosphere sometimes falls below the mean by as much as 0·004 or 0·005.

I will not cite any analyses made before the year 1841. In that year Dumas and Boussingault found it necessary to resolve by experiment the doubt as to whether the true proportion of oxygen in the air were exactly one-fifth, or were about twenty-one per cent, or were a variable quantity. If this was the uncertainty as to the mean of multitudes of analyses, it is obvious that we can by no means attribute to a single analysis a degree of precision sufficient to aid in the present inquiry. But in that year, Dumas and Boussingault used a new method of analysis, by means of which sufficient accuracy was obtained, and proposed an elaborate system of analyses on air collected simultaneously at different places. Lewy went to Copenhagen to take part in this system, carrying with him apparatus from the laboratory of Dumas and Boussingault. He had the coöperation of Oersted, and his results were communicated to the Academie des Sciences by Dumas and Boussingault. Four of his results on four samples of air, collected at sea on the voyage to Copenhagen, showed a proportion of oxygen as low as 0·2045.

Regnault's results will command entire confidence. A sample collected in the Bay of Algiers, June 5, 1851, gave 0·2042 and 0·2040 oxygen. A sample collected in the Bay of Bengal, February 1, 1849, gave 0·2046 and 0·2045 oxygen.

Jolly has used a new method equally accurate with the common process, by explosion with hydrogen, and very valuable as confirming the latter. He measures the tension of a confined volume of air while it is at the freezing-point. He then absorbs the oxygen from this air by means of a copper spiral heated by electricity, and again measures the tension at the freezing-point. The absorption and measurement are repeated till no more absorption takes place. A sample of air collected at Munich, June 15, 1877, gave 0·2053 oxygen, one collected July 19, gave 0·2056, and one collected November 10, gave 0·2056. Also at six other dates during the same months he found the amount of oxygen in the air less than 0·207.

A sample taken at this place, September 20, 1878, gave 0·2049 and 0·2049 oxygen. A sample taken February 26, 1879, gave 0·2045. The other analysis of this sample was lost by the accidental use of hydrogen containing a little air. But even this analysis, which of course gave the proportion of oxygen too high, gave only 0·2049.

The analyses of Macagno, at Palermo, made by absorbing oxygen with pyrogallol, I forbear to cite.

It is difficult to resist the conclusion that these analyses show that sometimes the deficiency of oxygen observed in the atmosphere at such latitudes as fifty-two, forty-eight, and forty-two degrees, may amount to 0·004 or 0·005. Then we must either suppose that not very seldom there might be observed within the tropics immense volumes of air in which the deficiency should be several times as great as this, or we must abandon the hypothesis in question.

If processes of oxidation preponderate over processes of reduction within the tropics, there must be a transportation of organic matter from colder climates toward the equator, there to be oxidized, but

3. No such amount of transportation as is required by the hypothesis takes place through the air. For, in the first place, experiment has repeatedly shown that after a volume of air has been freed from carbonic acid, there is left in it but a minute trace of matter capable of undergoing oxidation. Now, if a given volume of air contained an amount of organic matter capable, in its oxidation, of absorbing from this volume of air 0·005 of oxygen; and if further this organic matter was as rich in hydrogen as is marsh gas, even then the carbonic acid produced in some of these experiments would have been ten times as much as the carbonic acid already existing in the air. And secondly, if the observed deficiency of oxygen in the atmosphere had been produced by the oxidation of organic matter previously contained in it, the missing oxygen would be replaced in part by the carbonic acid produced, which, on the most favorable assumption, would amount to half the deficiency of oxygen. But experiment has shown that no such amount of carbonic acid is ever found in air uncontaminated by local causes, though a very large number of determinations has been made.

4. The transportation of organic matter required by the theory does not take place by the waters of the globe. If Jolly's hypothesis is true a very large part of the organic matter returned to the air in the form of carbonic acid must be supposed to be dissolved or suspended in the water which flows from the land into the sea, to be brought by ocean currents to the equatorial parts of the ocean, and there to be at last oxidized.

It may be noticed that this supposition would permit us to explain the removal of oxygen from the air without the restoration of a corresponding volume of carbonic acid to the same volume of air, by assuming that the oxidation takes place in the waters of the ocean while near the equator, but that the carbonic acid there produced is restored to the air but slowly, and therefore is not restored to the volume of air which afforded the oxygen.

Now, if this supposed mechanism of oxidation is not consistent with observed facts, the theory that the atmosphere within the tropics sometimes shows a deficiency of oxygen produced by the preponderance of processes of oxidation over those of reduction must be dismissed from consideration. My own knowledge is far from sufficient to enable me to assert that the hypothesis is disproved by facts already observed. But I may mention some of the points in which the theory may be compared with facts capable of easy observation, or perhaps already observed. Those who are familiar with observations on the chemistry of sea-water will be able to judge whether the hypothesis is not overthrown by these facts thus compared.

In the first place, if the supposed process of oxidation is the actual process, it must obviously be about as regular and invariable as the motion of rivers and ocean currents. A vigorous withdrawal of oxygen from the superincumbent air must then go on constantly within certain areas of the ocean. Whenever a volume of air is becalmed over such an area, so that the cause may operate for some time on the same air, such air should be highly deficient in oxygen. Now, can we find any evidence that air over some parts of the tropical oceans is specially deficient in oxygen whenever the winds are slight? If the evidence is of the opposite nature, Jolly's hypothesis lacks confirmation.

Again if the supposed oxidation takes place in the water, a somewhat rapid transfer of oxygen must go on between the air and the water. In the regions in question, whenever the sea is still, then there must be a falling off in the quantity of oxygen at different depths in the ocean. The contrast in this respect between equatorial waters and those at forty-five degrees of latitude ought to be capable of observation. A collation of results already obtained may perhaps afford a decisive test of the theory.

In the third place, if the supposed oxidation takes place through the waters of the sea, the retention of the carbonic acid produced is somewhat protracted. Determinations of carbonic acid in the air are very numerous, but no observer has yet found normal air containing one or two hundredths per cent of carbonic acid more than the average. Then, even when air is exposed long enough to oxygen-absorbing water to lose 0·005 of oxygen, it does not gain a noteworthy amount of carbonic acid. Now if the carbonic acid produced is thus retained, the water of some parts of the equatorial seas must be very abundant in carbonic acid. There must be a gradual diminution toward the poles; and further, within all moderate latitudes, there can be no equilibrium between the tension of carbonic acid in the air and that of carbonic acid in sea-

water. If facts do not agree with these deductions, the supposition that a large part of the processes of oxidation on the surface of the globe takes place in sea-water within the tropics is contrary to the facts.

In the fourth place, it is doubtful whether rivers carry any such amount of organic matter as is required by the theory. Determinations of the amount of oxidizable matter contained in the water of rivers have been chiefly limited to the water supply of towns. But some observations have been made on the water of the Nile. Tidy found by the permanganate process that 0·23 grain of oxygen was given up to a gallon of the water of this river. If we take this result to represent the amount of oxygen absorbed by river water *after the water reaches the tropics*, we shall concede much for argument. Such water could remove 0·001 oxygen from about ten times its own volume of air. Of course it is difficult to suppose that the consumption of oxygen can be localized in a small volume of air. Now, if such water be diluted with sea-water, and if it absorbs oxygen from a hundred times its volume of air, through several degrees of latitude, and if the deficiency of oxygen to be explained is several times 0·001, it is hard to believe that the cause is sufficient.

5. It is very doubtful whether the whole consumption of oxygen on the globe would account for the observed deficiencies of oxygen, even if we suppose this total consumption for a certain short period to be taken from one and the same small volume of air.

Dumas and Boussingault made an approximate estimate of the amount of oxygen used in a century by all process of oxidation. If we take this estimate we shall find that all the oxygen absorbed from the air in a week, if taken from the same volume of air covering but half a square degree of the earth's surface, and containing only the lower third part of the atmosphere, would produce in this limited volume a deficiency of oxygen of but one-eighth of one per cent. But we have to account for deficiencies several times as large, and we cannot suppose the consumption so limited to a small volume. Then the theory fails to agree with the facts.

At the foundation of the hypothesis which I have suggested to account for the observed deficiencies in the oxygen of the atmosphere, there lies the assumption that in a vertical column of a mixture of two gases of different densities, there is a tendency to the accumulation of a greater proportion of the heavier gas toward the bottom, and of a greater proportion of the lighter toward the top. There has not yet been obtained any direct experimental evidence in favor of this theory of

Dalton. Although the assumption is a simple, and, I think, certain inference from the known principles of mechanics as applied to gases, it is desirable that experimental evidence should be supplied. I have planned two forms of apparatus and two series of experiments for this purpose; but the making of a more perfect eudiometric apparatus than had heretofore been used, the carrying on a series of daily analyses in duplicate of samples of air collected at this place, and the providing for the collection of samples at other parts of the continent, have used so much of my time and income that so far it has been impossible to carry out these plans. I hope before long to supply this deficiency.

ART. LVI.—*Lower Silurian Fossils in Northern Maine;* by W. W. DODGE.

THE writer found graptolites in black shale in No. 3 township, of Range VII, Penobscot county, Maine, in September last. The fossils are, for the most part, mere bright films upon the dark rock, and in the small quantity of material brought away, but one or two individuals are sufficiently distinct and entire for identification. The fragments are of at least four varieties; the *Diplograptus* type predominates.

The most complete specimen is one of *Diplograptus pristis*, but of this the upper end of the axis is broken away. The cellules are about sixteen to an inch in each rank. Instead of narrowing gradually from end to end, as the drawings usually represent, the stipe retains its full width for an inch and a half and then its edges approach each other rapidly in the next half inch toward the solid, acicular radicle.

A clearly-marked fragment, three-eighths of an inch long, is of a width only half that of the preceding, the axis is much more distinct, the cellules, twenty-four to an inch on each side, although separated from one another nearly to the base by a rounded interval of about one-third their own width, are so shaped, with the denticle turned inward, that the appearance of serration in the stipe is subordinate to its linear, parallel-edged aspect. The general shape of what is visible is suggestive of *Graptolithus ramosus*, although no bifurcation appears. Close beside this is a branching fragment upon which no cellules are discernible, probably its stem.

One or two small, broadly-ovate shapes, perhaps *Phyllograptus*, and a few long, slender stems not sufficiently characteristic, or too incomplete, for their relations to be ascertainable, conclude the list of forms at present in hand.

The shale in which these remains are embedded is probably

to be referred to the level of the Utica slate or the Hudson River formation.

The locality is on the north side of the Wassatiquoik River, about a mile west of the East Branch of the Penobscot. The road to Katahdin Lake crosses the southern slope of Wassatiquoik Mountain (the eastern and smaller of the two so-called, the one which stands in Range VII upon the line between Nos. 3 and 4), while the river of that name runs at its foot. The shale is at the base of the hill on the eastern side—under its lee, with reference to glacial erosion.

The occurrence of fossiliferous rocks here is interesting as helping to correlate the Maine formations with the better understood Canadian strata, and also as narrowing the circle of known fossil-bearing beds about the Katahdin granite, whose position and age may sometime be determined by its relations to them, when a point of contact is found. Graptolites have been found in New Brunswick in that great belt of strata mapped as extending southwestward from the Bay of Chaleurs, with granite bands on its southeast side.*

The readiest cleavage of the thinly-layered shale which holds the above described fossils, is at 30° across the plane in which they lie. There is noticeable uniformity in the position of the long, slender forms, but the means is not at hand of determining through how great a thickness of accumulating strata the parallelism continued. The rock most nearly associated with the black shale is a black, or dark-blue, very hard, thick-bedded slate, of conchoidal fracture, sometimes semi-translucent in thin flakes. Another rock was too deeply weathered for examination with such tools as could be improvised. A coarse "greenstone" forms a ledge near by; and the presence of intrusives doubtless accounts for the condition of the flinty-looking slates. The only rock noticed in the three miles to the westward is a dull, greenish, hydrous-looking eruptive, mostly in boulders. Water-worn pebbles in the vicinity, apparently of this kind, are streaked with dull red, and show many cavities.

The nearest observed outcrop to the eastward is of slate with an easterly dip, on the left bank of the East Branch, near the water at its summer level, about opposite the mouth of the Wassatiquoik River. This is a mile and a half north of the Hunt farm, two miles east of which the road crosses a slate ledge where the strata dip to the westward. The outcrops of this slate along the East Branch have been examined by different observers, and its strike and dip at many points recorded.†

* J. W. Dawson, Acadian Geology, 1878, supplement to second edition, p. 78.

† C. T. Jackson, Second Annual Report on the Geology of the Public Lands of Maine and Massachusetts, 1838, pp. 20–24; C. H. Hitchcock, Agric. and Geol. Maine, 1861, pp. 392. 393.

One of the most noticeable facts connected with the presence of this rock between Molunkus and Sherman, along the post-road from Mattawamkeag to Patten, is the large amount of clear-white, fine-grained quartz rock scattered by the roadside.

The road from Sherman (No. 3, of Range V), to the East Branch at the Hunt farm, gives a good line of section nearly at a right angle across the line of strike there prevalent, and by comparison of the dips near the road and elsewhere, it seems to cross not less than four anticlinals and five synclinals. The western portion of the road is through woods. There is a large exposure of nearly vertical beds on the west side of Swift Brook. Between the brook and Sherman, a distance of five miles through partially cleared country, the road crosses four long ridges of high land, whose direction is that of the strike of the underlying rocks. Upon the hills the strata crop out occasionally, and in the valleys between flow small streams at regular intervals of a little over a mile from each other. On the hill just south of the village of Sherman, and near the line between Nos. 2 and 3, the slate shows a high dip westward.

Glacial.—The parallel courses to which so many of the long, narrow lakes and large and small streams of the northern part of Maine conform, appear to indicate the undeviating direction of primary glacial erosion in that region. The course of transported bowlders agrees well with this, as in the case of the limestone *in situ* in No. 4, R. IX,* observed in scattered bowlders upon the Wassatiquoik and at Whetstone Falls, on the East Branch, in No. 2, R. VII.† The uniform shaping of resistant ledges, such as may be seen at Mt. Kineo, and as is recorded of the slates along Webster Stream and at Grand Lake,‡ indicates in a general way the direction of the force exerted. The glacial striæ, as reported, appear to be more than usually divergent. To the two localities of the occurrence of granite bowlders from an unknown source named by Professor Hitchcock—north end of Churchill Lake in No. 9 of R. XII,§ and No. 5 of R. VIII,‖—may be added the site of one, high on the hillside above the East Branch opposite the Wassatiquoik. The granite pebbles in the bed of the Wassatiquoik at the dam, four miles above its mouth, may belong to the Katahdin mass, but the extent of the area occupied by this has not been definitely determined. The "porphyry" on Soper Brook,¶ in No. 8 of R. XII, may well be the source of the pebbles of porphyritic black felsite with quartz grains found at this dam.

* Agric. and Geol. Me., 1862, p. 321. † Ib. 1861, p. 393.
‡ Thoreau, Maine Woods, pp. 262, 277. § Agric. and Geol. Me., 1861, p. 411.
‖ Ib. p. 401. ¶ Ib. p. 411.

Cambridge, Mass.

ART. LVII.—*A Contribution to Croll's Theory of Secular Climatal Changes;** by W. J. McGEE, of Farley, Iowa.

BRIEFLY stated, Dr. Croll's theory of secular changes in terrestrial climate indicates that during periods of high eccentricity in the earth's orbit the hemisphere whose winters occur in aphelion suffers, through the intervention of physical and meteorological agencies, a diminution of temperature, while on the opposite hemisphere the temperature is correspondingly augmented.† It is the object of the present communication to direct attention to certain meteorological relations tending to produce such an effect, which appear to have been heretofore overlooked.

All extensive series of meteorological observations which have been examined by the writer indicate the existence of a general law, which may be expressed by the proposition: *Any increase in annual or diurnal thermometrical range is accompanied by a diminution in mean temperature.* Aside from the collocation of a portion of the results of an elaborate meteorological survey, for the purpose of establishing an empirical coefficient indicating the efficiency of the law in absolute measure, no discussion of this proposition will be here offered.‡

The accompanying table I is based upon Charte III of W. H. Dove's "Verbreitung der Wärme auf der Oberfläche der Erde,"§ and exhibits temperatures along the meridians passing through the Atlantic Ocean (long. 20° W.) and Central Asia (long. 120° E.).

TABLE I.

Temperatures at 20° W. and 120° E. of Greenwich.

Latitude.	Long. 20° W.			Long. 120° E.		
	January.	July.	Range.	January.	July.	Range.
0°	+79·2°	+77·0°	− 2·2°	+77·0°	+81·5°	4·5°
20	70·0	77·0	+ 7·0	60·6	81·5	20·9
40	55·9	68·0	12·1	26·4	75·4	49·0
60	36·0	57·2	21·2	−34·0	63·5	97·5
Polar circle	26 2	45·5	19·2	40·0	60·1	100·1
Mean	68·4	73·3	4·9	53·4	78·8	25·4

* Read before the Iowa Academy of Sciences, June 25th, 1880, and printed in brief abstract in the Proceedings, vol. i, pt. 1, p. 24. Read before the American Association for the Advancement of Science at Cincinnati, August 22d, 1881.

† Lond., Edinb. and Dublin Phil. Mag., Aug., 1864, Feb., 1867, etc. "Climate and Time," Edinburgh and New York, 1875.

‡ Cf. Proceedings American Association, vol. xxix, Boston Meeting, 1880, p. 486, *et seq.*

§ Berlin, 1852.

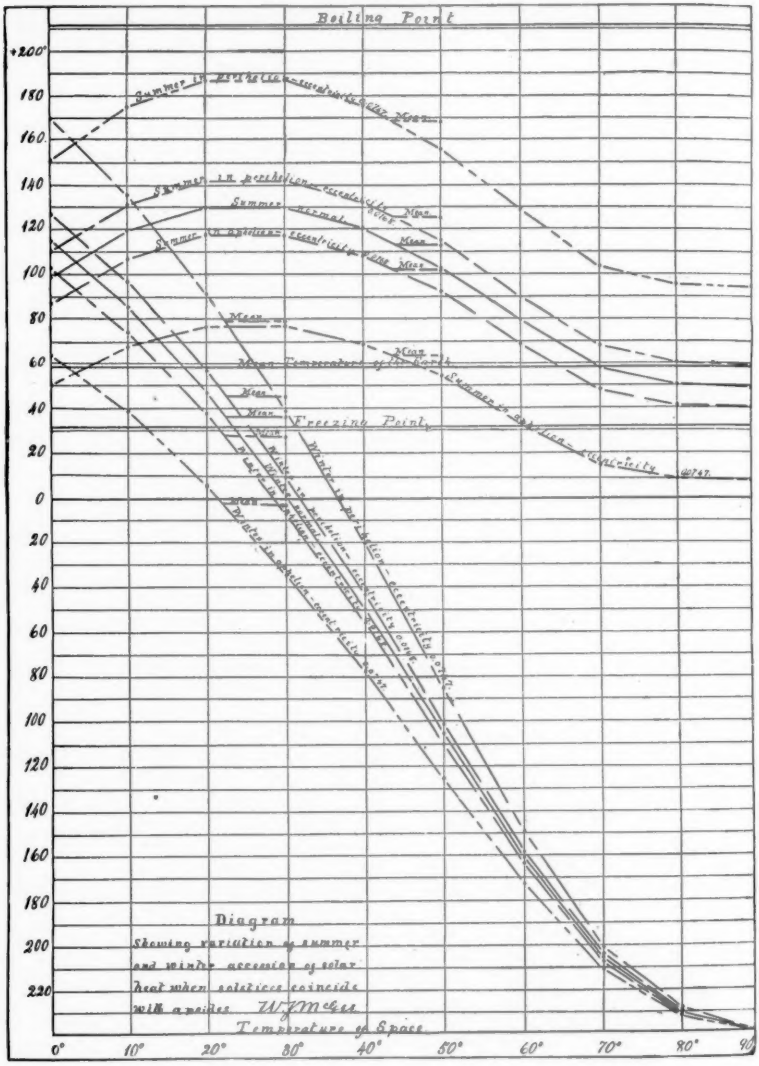


Diagram
Showing variation of summer and winter accession of solar heat when solstices coincide with apsides W J McGee

In computing the means the following coefficients were employed:

Latitude.	Coefficient.
0°	1·000
20	·658
40	·357
60	·134
Polar circle	·074

The mean annual temperatures are 70·8° and 66·1° respectively. It thus appears that the mean temperature is 4·7° lower and the thermometrical range 20·5° greater over the land-meridian than over the water-meridian; which ratio yields a coefficient of diminution of 0·23° for each degree of increase in range. For the present this ratio may be assumed to remain constant.

When the solstices are at right angles to the apsides the amount of light and heat received from the sun by either hemisphere during winter or summer is exactly equal to that received by the opposite hemisphere during its corresponding seasons. The amount so received may be denominated the *normal accession*. When, however, the solstices coincide with the apsides that hemisphere whose winters occur in aphelion while its summers occur in perihelion receives a less than normal amount of light and heat in winter, and a greater than normal amount in summer, owing to the variation in the earth's distance from the sun at these seasons. *If, then, terrestrial temperature is a function of solar accession*, the annual thermometrical range on the hemisphere so situated must be greater than the normal; while at the same time the thermometrical range must be diminished on the opposite hemisphere. Manifestly, too, any increase in the eccentricity of the terrestrial orbit must intensify this effect, since solar accession varies as the square of the solar distance.

In table II the solar accession in winter and summer when the solstices and apsides coincide and the eccentricity of the terrestrial orbit is as at present (0·0168), is compared with the normal, values being expressed in degrees Fahrenheit. Table III exhibits like values for an eccentricity of 0·0747, such as occurred 850,000 years ago according to Croll's calculation from LeVerrier's formulæ.* Both tables are graphically depicted in the accompanying diagram.

These tables were computed as follows:—The relative solar

* "Climate and Time," p. 319. Stockwell computes the maximum eccentricity to be 0·0693888 ("On the Secular Variations of the Elements of the Orbits of the Eight Principal Planets," Smithsonian Contributions to Knowledge, No. 232 (1872), p. xi); but his memoir was not accessible when the table was prepared. The slight diminution in normal accession accompanying increased eccentricity is also neglected.

Table II.

Variation in summer and winter accession of solar heat when solstices coincide with apsides, with eccentricity of 0·0168. Aphelion intensity, ·967; perihelion intensity, 1·034.

Latitude.	WINTER.						SUMMER.						Increase in Annual Thermometrical Range.	
	Normal Accession.	Aphelion.		Perihelion.		Difference between Aphelion & Perihelion Accession.	Normal Accession.	Aphelion.		Perihelion.		Difference between Aphelion & Perihelion Accession.	Above Normal.	Above Opposite Hemis.
		Accession.	Below Normal.	Accession.	Above Normal.			Accession.	Below Normal.	Accession.	Above Normal.			
0°	+114·8°	+103·2°	11·6°	+126·8°	12·0°	23·6°	+98·8°	+87·7°	11·1°	+110·3°	11·5°	22·6°	23·1°	46·2°
10	85·4	74·7	10·7	96·4	11·0	21·7	119·2	107·4	11·8	131·2	12·0	23·8	22·7	45·2
20	47·1	37·8	9·3	56·8	9·7	19·0	129·9	117·7	12·2	142·4	12·5	24·7	21·8	43·7
30	1·3	−6·7	8·0	9·5	8·2	16·2	129·9	117·7	12·2	142·4	12·5	24·7	20·4	40·9
40	−49·9	56·1	6·2	−43·5	6·4	12·6	120·6	108·7	11·9	132·6	12·0	23·9	18·3	36·5
50	105·9	110·4	4·5	101·4	4·5	9·0	103·6	92·3	11·3	115·1	11·5	22·8	15·9	31·8
60	161·1	163·8	2·7	158·5	2·6	5·3	79·2	68·7	10·5	89·9	10·7	21·2	13·3	26·5
70	206·1	207·4	1·3	205·0	1·1	2·4	57·8	48·0	9·8	68·0	10·2	20·0	11·2	22·4
80	230·1	230·5	0·4	229·8	0·3	0·7	50·7	41·1	9·6	60·5	9·8	19·4	10·1	20·1
90	239·0	239·0	0·	239·0	0·	0·	49·8	40·2	9·6	59·6	9·8	19·4	9·7	19·4
Mean.	+36·1	+27·1	9·0	+45·5	9·4	18·4	+113·3	+101·9	11·4	+125·5	12·2	23·6	21·0	42·0

Table III.

Variation in summer and winter accession of solar heat when solstices coincide with apsides, with eccentricity of 0·747. Aphelion intensity, ·856; perihelion intensity, 1·115.

Latitude.	Winter.						Summer.						Increase in Annual Thermometrical Range.	
	Normal Accession.	Aphelion.		Perihelion.		Difference between Aphelion & Perihelion Accession.	Normal Accession.	Aphelion.		Perihelion.		Difference between Aphelion & Perihelion Accession.	Above Normal.	Above Opposite Hemis.
		Accession.	Below Normal.	Accession.	Above Normal.			Accession.	Below Normal.	Accession.	Above Normal.			
0°	+114·8°	+64·0°	50·8°	+169·6°	54·8°	105·6°	+98·8°	+50·1°	48·7°	+151·3°	52·5°	101·2°	103·4°	206·8°
10	85·4	38·7	46·7	135·7	50·3	97·0	119·2	67·6	51·6	174·8	55·6	107·2	102·1	204·2
20	47·1	6·2	40·9	91·5	44·4	85·3	129·9	76·8	53·1	186·9	57·0	110·1	97·7	195·4
30	1·3	−34·4	35·7	38·5	37·2	72·9	129·9	76·8	53·1	186·9	57·0	110·1	91·5	183·0
40	−49·9	77·0	27·1	−20·6	29·3	56·4	120·6	68·8	51·8	176·2	55·6	107·4	81·9	163·8
50	105·9	125·0	19·1	85·3	20·6	39·7	103·6	54·3	49·3	156·6	53·0	102·3	71·0	142·0
60	161·1	172·3	11·2	149·1	12·0	23·2	79·2	33·4	45·8	128·6	49·4	95·2	59·2	118·4
70	206·1	211·0	4·9	201·0	5·1	10·0	57·8	15·0	42·8	103·6	45·8	88·6	49·3	98·6
80	230·1	231·4	1·3	228·7	1·4	2·7	50·7	9·0	41·7	95·6	44·9	86·6	44·6	89·3
90	239·0	239·0	0·	239·0	0·	0·	49·8	8·2	41·6	94·7	44·9	86·5	43·2	86·5
Mean.	+36·1	−3·5	39·6	+78·8	42·7	82·3	+113·3	+63·1	50·2	+168·2	54·9	105·1	93·7	187·4

intensity at the various terrestrial latitudes has been calculated by Meech* and expressed in arbitrary units, each representing $\frac{1}{80}$ of the intensity under the equator at the time of the vernal equinox. The mean for the whole earth, in the same units, is 66·73. Dove had previously, as a result of an elaborate series of observations, determined the actual mean temperature of the earth to be about 58° F. According to Herschel's determination of the temperature of space (which agrees pretty closely with that of Pouillet), or −239°, this temperature is 297° higher than that of stellar space. Each of Meech's units is, therefore, so far as the whole earth is concerned, equal to 4·45° F. This coefficient has also been assumed to be constant; and the intensities, both normal and corresponding to the different degrees of eccentricity, have been reduced to degrees Fahrenheit by its use. The means were computed by the use of the following coefficients:

Latitude.	Coefficient.
0°	1·000
10	·826
20	·658
30	·500
40	·357
50	·234
60	·134
70	·060
80	·015
90	·001

The increase in thermometrical range beyond normal in tables II and III is 21·0° and 93·7° respectively. Making use of the coefficient already determined (0·23°), it appears that these values are equivalent to a diminution in mean temperature over the hemisphere whose winters occur in aphelion of 4·83° and 21·55° respectively, and to a like increase in the temperature of the opposite hemisphere.

It may be added that aside from the specific relations pointed out, the alternate free summer precipitation and rapid winter congelation of seasons varying so widely in temperature would certainly facilitate the formation and conservation of glacier ice.

In the foregoing pages two variable factors have been assumed to be constant. These are the ratios (1) between increase in thermometrical range and diminution of mean temperature, and (2) between solar accession and terrestrial temperature. The first of these ratios appears to augment rapidly with diminution of temperature, as may be seen from a glance at

* "On the Relative Intensity of the Sun's Light and Heat," vol. ix of Smithsonian Contributions to Knowledge, 1856.

table I—indeed comparison of a larger number of observations yielded a larger coefficient. Accordingly, since the temperatures dealt with in tables II and III are collectively lower than those collocated in table I, the use of the coefficient adopted is probably perfectly safe. With respect to the second ratio, the lack of correspondence between observed and computed temperatures indicates that results obtained by its use are excessive. Comparison between observed and computed temperatures will, however, afford the means of eliminating errors arising from this cause. Thus, the actual diminution of terrestrial temperature from equator to pole is about 30° according to Dove, while it would be about 212° if proportional to the solar accession as computed by Meech. Reducing the figures 4·83° and 21·55°, derived from tables II and III respectively, in the ratio of 212 : 80, yields 1·82° and 8·13° as tolerably trustworthy values for the diminution of mean temperature effected by the operation of the law stated at the outset.

Applying the first of these values to the earth in its present status, it would appear that the temperature of the southern hemisphere ought to be about 3·5° lower than that of the northern. The approximate coincidence between this result and those derived from observation strengthens the conviction that the principles detailed in the foregoing pages must be valid. Applying then the second value to the earth when the eccentricity is near its superior limit, it appears that the hemispheres should vary in mean temperature by no less than 16°—that secular summer should prevail in one, while the other was enshrouded in the snows of its secular winter. The importance of the agencies described will perhaps be more manifest when it is borne in mind that during its secular summer more solar heat and light is received by a hemisphere in winter than in summer, while on the opposite hemisphere the solar accession is no less than $1\frac{1}{15}$ times greater in summer than in winter.

Art. LVIII.—*The Stereoscope, and Vision by Optic Divergence;* by W. LeConte Stevens.

[Continued from page 362.]

In a previous article* it has been shown that Brewster's theory of binocular perspective is insufficient to explain vision through the stereoscope when the visual axes diverge. It takes account of only one of several elements which combine to determine the judgment of distance, and the significance of this

* This Journal, Nov., 1881.

should be referred to the sensation of muscular strain rather than to the intersection of visual lines.

The effect of varying the tension of the rectus muscles of the eyes in modifying the estimate of relative distance has been applied in Wheatstone's pseudoscope* and in his reflecting stereoscope, though no reference in this connection has been distinctly made to anything beyond variation of convergence. The following experiment is not difficult. Upon a large sheet of paper a series of vertical parallel lines are drawn, 50^{mm} apart; the last line of this series forms the first of a second series 60^{mm} apart, and in like manner this introduces a third series 70^{mm} apart. Gazing at the first series, as if regarding a remote object, the paper being 1^{m} distant, the images of the lines are soon combined by diminished convergence. Passing slowly to the second series, the convergence is still farther diminished, and it passes into divergence when the third is successfully combined. The apparent distance of the first series I estimate at $2^{m}{\cdot}5$, of the second about 3^{m} and of the third about $3^{m}{\cdot}5$. By intersection of axes, the first should be 6^{m}, the second infinity, and the third -6^{m}, my interocular distance being 60^{mm}. The experiment may be varied in many ways; different observers form different estimates of distance, but I have found none who succeeded in attaining divergence thus without observing an apparent recession of the external image.

To ascertain whether divergence of axes is unconsciously practiced in the use of the stereoscope, I examined 166 stereographs taken at random and found the foreground interval to vary between 60^{mm} and 95^{mm}, the mean being $72^{mm}{\cdot}9$. The average interocular distance for adults is a little less than 64^{mm}; to combine without the stereoscope, therefore, divergence is nearly always necessary. To ascertain the mean deviating power of the lenticular prisms used in the best instruments, 30 pairs were obtained through the courtesy of Mr. H. T. Anthony, of New York. With but slight variation, the focal length was found to be $18^{cm}{\cdot}3$. Mounting each pair in succession, parallel rays 64^{mm} apart were transmitted and received upon a screen $18^{cm}{\cdot}3$ distant. The mean interval between points of light caught on the screen was $79{\cdot}1^{mm}$; hence if rays be sent from corresponding stereograph points, separated by a wider interval than this, they will be not quite parallel after emergence from the prisms, and the eyes must diverge to receive them; 80^{mm} may be taken as a limit beyond which most persons will find divergence necessary if binocular combination in the stereoscope is successfully attained. As this limit is not unfrequently exceeded, axial divergence, unconsciously attained, is quite common, though

* Phil. Transactions, 1852, part I; or, Phil. Magazine, 1852, pp. 506–523.

in extent it rarely exceeds 2° or 3°. I have attained 7°, and Helmholtz * gives 8° as his limit. Several persons of my own acquaintance have been found able to secure divergence without the stereoscope, and their estimates of the apparent distance, size and motion of the external image under various conditions have not differed much from my own.

In the discussion of normal binocular vision, the expression "point of sight" may be applied theoretically to the intersection of optic axes. Its *apparent* position, though not mathematically determined, may be estimated with more or less error, according to the skill of the observer. But in discussing the stereoscope such a definition has to be totally abandoned. The point of sight then is only the point in space to which the observer mentally refers the binocular combination of images formed on corresponding retinal points, where the visual axes, whether convergent, parallel or divergent, meet the retinas. Its apparent position has to be estimated, not determined by intersection of lines. In this estimation the relation between the visual axes is only one of a number of elements that are combined in the formation of a judgment, whether vision be normal or abnormal. Even if stereographs are selected from which physical perspective is in great measure eliminated, the optic angle may be negative; and, when positive, its effect is still antagonized by the disturbance of coördination between focal and axial adjustments, or by the observer's unconscious recognition of the circumstances under which he has been accustomed to view an object of the kind represented. A mountain will never be judged to be so near as a mere diagram, even though the axial relations be similar in viewing the pictures separately. In the stereoscope before me I place a pair of conjugate diagrams representing a skeleton cone, alternately approximating and separating them, in a transverse vertical plane, so that the optic angle varies between $+8°$ and $-3°\ 45'$. The apparent distance varies between 30^{cm} and 40^{cm}; if determined by the optic angle it should vary between $+43^{cm}$ and -92^{cm}.

The distance of the card remains constant, and tends to keep the focal adjustment so, while the eyeballs are rotating outward, tending to produce adaptation of focal adjustment to a greater distance, the two adjustments being usually consensual. We are in the habit of associating diminution of convergence with increase of distance of the object of sight. As long as the eyeballs continue rotating outward, therefore, the object appears to recede and to enlarge correspondingly, the recession being fastest during the change from marked convergence to parallelism. It does not seem possible to express this apparent rate in mathematical terms.

* Optique Physiologique, p. 616.

The experiment just described does not imply any unusual conditions in the stereoscope except that the higher value, 8°, given to the optic angle is greater than usual. Assuming $72{\cdot}9^{mm}$, the mean already found for the stereographic foreground interval, the corresponding angle of convergence after allowing for deviation of rays is a little less than 2°; the intersection of axes is hence still far from the point to which the focal adjustment is adapted. This fact explains the difficulty experienced by so many persons in obtaining distinct vision through the stereoscope, especially those who have passed beyond middle age and lost in great measure the power of focal accommodation.

Most of the stereographs in common use are pictures in which physical perspective is strong. When these are properly mounted and viewed in the stereoscope the chief advantage gained by use of this instrument seems to be that it necessitates variation in the relation between the optic axes, in order that perfect binocular combination of the different parts of the superposed retinal images be secured in the subjective Cyclopean,* or combined binocular, eye. If there is perfect superposition of retinal points on which the foreground of the stereograph is imaged, there is necessarily imperfect superposition of those on which the background is imaged. If the attention is then given to the background, slight outward rotation of the eyeballs is necessitated, and this is habitually associated with recession of the point of sight. Whether with axial divergence binocular relief is instantly perceptible, as in Dove's experiments with axial convergence, by illumination of the stereograph with the electric spark, I am unable yet to say; I hope to test this at no distant day. It should be so according to Professor LeConte's theory of binocular perspective.†

What has been generally given and accepted as the mathematical theory of the stereoscope applies strictly, but only to the relations involved in taking the photographs with cameras appropriately placed, so that the axes of the lenses converge upon some point of the object pictured. When the negatives are once fixed and proofs from them so mounted that corresponding points from the pair are focalized upon corresponding retinal points for the observer who binocularly combines them, with or without the stereoscope, the relation between the different parts of the fields of view combined undergoes no sensible variation, real or apparent, except between the limits fixed by difference between the stereographic intervals in the background and foreground respectively. If the eyes are comfortable, after binocular combination is attained, it makes little

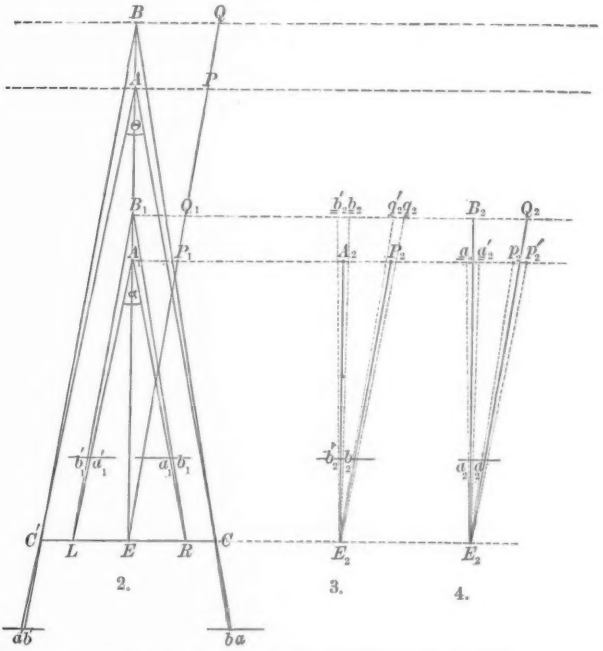
* This Journal, III, vol. i, p. 33 et seq.
† This Journal, III, vol. ii, p. 3.

difference whether, at a given moment, the visual axes are convergent, parallel, or divergent. The combined external image as a whole is made to appear nearer by convergence and farther by divergence, but this has no perceptible effect upon the ratio between the distances of its different parts. Though the distinctness in separation between foreground and background is greatly enhanced by the slight variation in axial relations that is necessitated, the estimation of absolute distance is determined mainly by physical perspective, and by comparison of the picture with known realities to which it bears some easily recognizable relation. In the few cases where reversion of perspective is plainly produced by transposing the pictures composing the stereograph, it will be found that the difference between background and foreground intervals is large, and that some of the elements of physical perspective are relatively weak. I have examined a number of such transposed stereographs and found the effect in many cases to be not distinct reversion but rather confusion. Sometimes in one part of the picture reversion is noticeable, while in the rest there is only decrease in the apparent distance between background and foreground. The conflict between physical and physiological perspective results in a judgment not wholly in obedience to either; generally the former prevails, but the weakness of the residual effect is perceived by contrasting it with that obtained by squinting and thus reversing the sense of the physiological element. The judgment may be regarded as a compromise rather than an independent selection. In vision by divergence, and in vision through the stereoscope generally, the *binocular relief* is largely due to the *variable relation between the optic axes*, as different parts of the stereograph are examined; while the judgment of absolute distance is mainly due to *physical perspective and comparison with remembered realities*; it is modified by focal adjustment, and is in practice nearly, but not quite, *independent of the optic angle*. This remark would not apply if the optic angle were very large.

No diagrams can ever represent with perfect accuracy the apparent positions of objects seen in the stereoscope. If we neglect such disturbing influences as arise from conflict between focal and axial adjustments, and from difference between the optic angle and that between the camera axes when the pictures were taken, and also disregard the fact that the surface of the retina is curved while that of a photograph plate is plane, the following method perhaps will give the best results.

In fig. 2, let C and C′ be the centers of two camera lenses whose principal axes are as usual parallel, and a pair of secondary axes forming an angle, θ, in a horizontal plane, are di-

rected upon an object, A, in the foreground of a scene. Let E be the midpoint between C and C′; then EA is a median; on this prolonged let B be an object in the background. Parallel to C′C and to the vertical plane of the sensitized plates

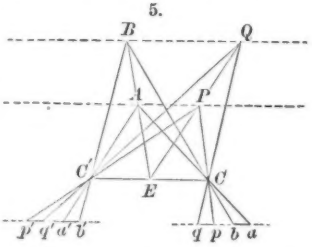


behind the lenses, let two planes be passed through A and B respectively. Let P and Q be any points on these planes, so related that the straight line QP passes through E. On the plates the stereoscopic displacements of the projections of B from those of A are $a\,b$ and $a'\,b'$; and it may be easily shown geometrically that the displacements of those of Q from P are each equal to $a\,b$. This is not shown in the drawing, but a glance at fig. 5 is sufficient.

Let E be midpoint also between a pair of eyes, R and L, in front of which the conjugate photographs are placed after being inverted, and let rays from them be so deviated by semi-lenses as to make $\alpha=\theta$. If the ratio of LR to C′C be known, the distances EA_1, EB_1, EP_1 and EQ_1 are determined. In binocular vision the direction of the object seen is always estimated from the position of the combined binocular eye, E, and is coincident with that of the median between the two visual

axes, but always somewhere in front.* This is universally true for normal eyes, as may be abundantly learned by experiment, whether the axes be convergent, parallel or divergent, and whether the median be at right angles or oblique to the interocular line, LR. In fig. 4, if E_2A_2 and E_2P_2 represent these medians, we have both direction and distance determined for these foreground points. To the right eye, B_1 (fig. 3) appears beyond and to the right of A_1 at an angular distance determined by the stereoscopic displacement, a_1b_1; to the left eye, beyond and to the left at an equal angular distance; to the binocular eye, E_2 (fig. 4), it is hence homonymously doubled at b'_2b_2. To secure single vision of it, the optic angle must be diminished, and through the rectus muscles this at once suggests to the mind increase of distance, producing at the same moment heteronymous doubling of the foreground point A_2, as in fig. 4. Similar remarks apply to P_2 and Q_2.

5.



If α be less than θ, as is often the case, this fact will cause the observer to estimate A_2 to be more distant than it is represented in the drawing, but by no means necessarily so distant as the actual vertex of α. If α be reduced to zero or become negative the sensation of still further change of muscular tension makes the apparent position of A_2 recede still more, and also that of B_2 in the same proportion; but in no case is this determined by intersection of visual axes except when $\alpha=\theta$. No one can have failed to notice the exaggeration of perspective in some stereoscope pictures, produced by making θ large while α is rendered small or negative by mounting the pair too far apart. This indeed was noticed by Wheatstone,† who approaches very near to the idea of possible optic divergence accompanying the perception of binocular relief, when he says, "but I find that an excellent effect is produced when the axes are nearly parallel by pictures taken at an inclination of 7° or 8°, and even a difference of 16° or 17° has no decidedly bad effect." His preconception that optic convergence, even though slight, is indispensable, prevented his apprehension of more than part of the truth. He states, as a remarkable peculiarity, that "although the optic axes are parallel, or nearly so, the image does not appear to be referred to the distance we should, from this circumstance, suppose it to be, but it is perceived to be much nearer." Such large angles as 17° are sel-

* This Journal, III, vol. i, p. 33 et seq.

† Wheatstone, Physiology of Vision, Phil. Mag., 1852, pp. 513–514.

dom resorted to at present. For taking stereographs of statuary, etc., the lenses of the binocular camera are not often more than 80^{cm} or 100^{cm} apart.

That muscular tension is more important than mere intersection of axes in affecting the judgment of distance and size may be shown by aid of Wheatstone's reflecting stereoscope. Having placed the two outline drawings, each 20^{cm} from its mirror so that a distinct combination is attained by axial parallelism, the judgment of distance is as definite as could be desired. Upon converging the axes strongly and giving attention successively to the two monocular images thus obtained, each appears greatly diminished in comparison with the binocular image just seen. Moreover, at the moment one of them is made an object of special attention, the other grows slightly larger. We have thus images of three apparent sizes, according to the degree of muscular tension with which they are separately regarded, while the visual angle remains constant. The visual axes are converged until their intersection is not more than 5^{cm} or 6^{cm} off, and the illusive impression is that each image is in the direction of its own axis much beyond the intersection. But in fact, being monocular images, the direction of the center of each is that of a secondary axis, the right eye perceiving that on the right, instead of the left. Since the optic center and center of rotation are about $6{\cdot}6^{mm}$ apart, the former being displaced toward the nasal side during the experiment, the two secondary axes meet at a very distant point in the rear. While the distance of the monocular image is indeterminate, it is judged easily enough to be *not* at the vertex of either the apparent or real angle determined by the meeting of axes. The experiment is very striking and is not difficult. We have a binocular image, of little more than natural size, with clear judgment of distance, as the result of axial parallelism; two monocular images, of diminished and separately variable size, with very uncertain judgment of distance, as the result of axial convergence, the principal and secondary axes being subjectively interchanged. The apparent diminution in size of the monocular images may be easily observed by crossing the eyes, while holding in front a card on which a sharply defined outline is drawn. I may discuss this still further in a future paper.

No theory of the stereoscope that includes axial divergence is possible, unless we recognize the subjective combination of the two eyes into a single central binocular eye as the point of origin in all perceptions of direction, distance and form. What is essential for binocular vision is not any particular relation between visual axes but rather superposition of retinal images in the binocular eye. What seemed uppermost in the minds of Wheatstone and Brewster * was superposition of external vir-

* Wheatstone, Physiology of Vision, Phil. Mag., 1852, pp. 243 and 246. Brewster, on New Stereoscopes, Phil. Mag., 1852, pp. 17–26.

tual images by causing rays from two pictures to deviate and appear to come from one central combined external picture. This would exclude the possibility of optic divergence, but seems to be still the most generally accepted theory of the stereoscope. In securing dissimilar pictures of the same object by convergence of camera axes we secure the conditions for the perception of binocular relief by divergence of visual axes.

In the diagram attention is called to the identity in position between the optic center of the binocular eye and the only point through which lines can be drawn in such a way as to cause the stereoscopic displacement to be constant for projections of the points where these lines cut the foreground and background planes. This fact alone is enough to suggest that in vision through the stereoscope the midpoint between the eyes must be the point of origin from which distance and direction are to be perceived. A truth that was first recognized as a physiological necessity is thus confirmed by purely mathematical considerations.

The dissociation between focal and axial adjustments in forced convergence or divergence is at first troublesome and productive of indistinct vision, but this vanishes in great measure after a little practice in ocular gymnastics. If the eyes are comfortable we are apt to forget that the vision is abnormal, and to assume that conditions exist which belong only to normal vision. To ascertain what modifications are imposed by physiological conditions upon the generally accepted mathematical theory of the stereoscope has been the chief object of the present investigation.

New York, Sept. 16, 1881.

ART. LIX. — *On the relation of the so-called "Kames" of the Connecticut River Valley to the Terrace-formation;* by JAMES D. DANA.

SINCE the publication of my papers of 1875 and 1876 on the stratified drift of Southern New England treating especially of the character and effects of the flood closing the era of ice, large additions have been made to our knowledge of the terraces of the Connecticut Valley, and of some other parts of Northern New England, through the New Hampshire Report of Mr. Warren Upham, published in 1878.* In his Report, Mr. Upham describes in detail the stratified drift or terrace-

*Geology of New Hampshire, Part III, Chapter i, Modified Drift in New Hampshire, by Warren Upham, pp. 3–177. 1878. A synopsis of Mr. Upham's Report, by its author, was published in this Journal, vol. xiv, p. 459, 1877.

formation of the valley; gives the heights of the terraces above the river (and above mean tide) from careful levelings along its course, commencing near the source of the river in Connecticut Lake, 1618 feet above the sea; discusses the origin of the deposits and of their various features; and presents his very valuable topographical details on a map of the valley occupying a series of plates. Besides the ordinary stratified drift, Mr. Upham finds gravel ridges or deposits to which he applies the name "Kames." According to his observations, the "kames" were formed before the deposition of the beds of the terrace-formation and after that of the till or unstratified drift, so that they represent an intermediate stage in the progress of the era and call for special explanations.

The facts from the Merrimac Valley also are presented in a similar way, and with like deductions.

In the study which I had made of the Quaternary of Southern New England, and less perfectly of drift-phenomena elsewhere, I had been led to refer all the stratified drift above the till to the terrace-formation; and no later observations in river valleys had resulted in the discovery of any thing answering to Mr. Upham's "kames." During the past summer, I have been over the region of the Connecticut Valley described by Mr. Upham, in order to obtain a full understanding of his facts, so as to be able to incorporate them with the knowledge I had previously acquired, and I here give an account of what I observed, with my conclusions.

That the subject may be rightly apprehended, I preface my statement with a brief mention, first, of some of the general facts respecting the stratified drift-deposits which I had gathered from personal study, and, next, of the facts and deductions which are brought out by Mr. Upham with relation to the "kames."

I.—(1.) Scratched bowlders and till are almost uniformly absent from the valley terraces of New England and from the stratified beds that make the terrace-deposits. Exceptions occur where the underlying rocks having till over them come so nearly to the surface of any terrace that the till outcrops.

(2.) The layer of till of the hill-slopes is continued beneath the terrace deposits; showing that along the valleys the till with the bowlders was generally deposited first.*

* In the street adjoining my own house, in New Haven, a trench, excavated for a sewer, passed through ten feet of stratified drift, or of the terrace formation, and then opened into a deposit of gravel and scratched stones (including some bowlders of eight to ten cubic feet); and, below two or three feet of this kind of material, entered the Mesozoic sandstone of the region. This sandstone rises in a ridge, above the level of the terrace, 400 yards to the north of the excavation, and must have constituted both the shore and bottom of the valley-waters at the time of the deposition.

(3.) The stratified drift of the valley consists ordinarily of fine material below, and coarser toward or at the top; the bottom portion being commonly of clay or loam, or fine sand with frequently more or less clay; then, following this, layers of sand often fine, but often with more or less gravel; then above, toward the top in the upper fifteen or twenty feet, coarser gravel, and in some regions cobble-stone beds; an order of arrangement, which indicates—in accordance with ordinary hydraulic principles—that the flow of the depositing waters was, as a general thing, less rapid at the time of the early depositions, and most so during the later or that of maximum flood. Exceptions exist along those streams that were torrents, and sometimes at the mouths of tributaries to large streams.

An uppermost sandy layer, of two or three feet thickness, frequently exists, indicating that the ebb commenced in a lessened rate of flow.

(4.) The portion of the terrace formation in a river valley that is nearest to the river or adjoins the channel-way, may, and often does, consist largely of beds of coarse gravel or cobble-stones, while one or two hundred yards away from the river it is composed chiefly or wholly of beds of sand; the river-border deposits being thus coarse because of the sifting or assorting action of the stream in violent flow along its channel or against one or the other side of it. And the coarseness may diminish down stream, because of greater remoteness from the source of coarse material, and also because of a change in the rate of flow, producing less power of transportation and so allowing of a deposition of the sands drifted out above.

(5.) Terraces of different degrees of coarseness and of different heights were sometimes simultaneously made on opposite sides of a stream, owing to the different rates of flow in the waters along the two sides.*

* Along the middle one of three streams entering the New Haven Bay, called Mill River, coarse gravel and cobble-stone deposits characterize the New Haven terrace-formation all the way to the harbor; they are vastly coarser on the west side of the stream than on the eastern, and in the southern part of its course are most so along a more western line away from the present stream. Moreover, the deposits make a terrace on the *west* side of the stream of only twenty-five feet above mean-tide level, while on the *east* side, where the material is so much less coarse, they rise to a height of forty-three to forty-five feet, or the ordinary level for the New Haven plain at that distance from the Sound. Those coarsest beds were made under the sifting action of the violently flowing waters (the pitch of the stream for some miles back being eight to ten feet a mile), and hence, that is, because of the loss of the finer material in this way, the height attained on the side of most rapid flow was twenty feet below the normal height. Moreover, the violent waters were probably those of the nearing maximum stage of the flood; for the coarse gravel deposits (as various sections show) extend down but fifteen feet from the surface, and rest on beds of sand and fine gravel.

(6.) The terrace-formation of a large and broad valley was made mainly, not from what its river transported, but from the contributions of tributaries. Consequently, (*a*) the height of the maximum flood is best registered in terraces at the mouths of tributaries, and (*b*) where tributaries fail for long distances, there may be only low terraces; further, (*c*) the coarsest gravel beds should exist in the deposits about the mouths of tributaries, and especially in those made along the banks of the *main river* near these mouths, where the contributions were subjected to the sifting action of the swiftly flowing river.

(7.) The extent and height of the terraces made along any part of a valley depended not merely on the amount of contributed material, but also largely on the size and form of the valley. Where very wide and deep, like many lake basins, the deposits were generally sufficient to make only low or narrow terraces; where narrow, the flow of waters was sometimes, because of the diminished width, too rapid for any depositions; but where the valley, though narrow along the main channel had a broad region of ledges on either side that became overflowed when the waters were nearing their maximum depth, a high terrace might then have become of great width; for the shallow region favored deposition by offering resistance to the flow, and however wide needed little material to cover it. Just as this condition favored the making of a broad upper terrace, so it favored the making of a wide terrace at lower levels, especially if the flow of water continued long at those levels.

(8.) Ice-floes, bearing sand, gravel and bowlders, added to the transported material for the terrace-formation; and they should have been abundant during the breaking up, at the time of maximum flood. Being carried by the waters, their distribution of material would have taken place for the most part in accordance with the principles above explained.

II. Mr. Upham adopts in his New Hampshire Report, the view that the valley formations are deposits made by the flood from the melting glacier, and it appears from his explanations that he would accept without objection several of the above explanations. The points of discrepancy, however, are many and important. I cite here only those relating to the "*kames*," and mostly in the author's words. The term *modified* drift is used by him for *stratified* drift.*

Page 12. "The oldest of the deposits of modified drift are long ridges, or intermixed short ridges and mounds, composed of very coarse water-worn gravel or of alternate layers of gravel and sand irregularly bedded." "Their position is generally along the *middle* or *lowest parts* of the valleys." Wherever the

* I have avoided the term *modified*, because it is not known to express in all cases the truth, preferring the non-committal term *stratified*.

ordinary fine alluvium of any terrace occurs adjoining a kame, "it overlies or in part covers the kame deposits," the ordinary terraces being of later formation than the kames.

Page 43. Along the Connecticut, between Vermont and New Hampshire "from Lyme to Windsor, a continuous gravel ridge or kame extends 24 miles, along the middle and lowest portion of this valley, with its top 100 to 250 feet above the river." "Its material is gravel and sand in irregular obliquely-bedded layers, always showing an inclined, and in most cases a distinctly anticlinal or arched stratification. The gravel, which always forms the principal part of the ridge, varies in coarseness from layers with pebbles only 1 or 2 inches in diameter to portions where the largest measure 1½ or 2 feet. The fine kinds prevail." "The sand is usually coarse and sharp, well suited for masons' use; it occurs in layers of varying thickness up to one or two feet, but sometimes it is wholly wanting." "All the materials of this kame, and of its remnants along this valley, are plainly water-worn and stratified."

Page 44. "The most important feature of this kame, if we compare it with others in New Hampshire, is that along its entire extent it constitutes a single continuous ridge which runs by a very direct course nearly in the middle of the valley, having no outlying spurs, branches, parallel ridges, or scattered hillocks of the same material associated with it."

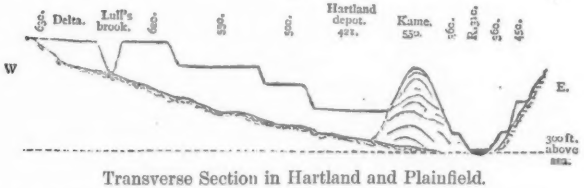
1.

Southern part of the "Kame," in Hartland and Windsor.

Page 45. "In calling this kame continuous from Lyme to Windsor, it is not meant to imply that it is now entire, since it has been frequently cut through and considerable portions swept away by the main river and its tributary streams; but that so much of it remains as to make it certain that it originally formed an unbroken ridge." The former southward continuation of the kame below Windsor is stated to be "probable though now shown by only a few fragments." Mr. Upham then mentions, on p. 47, facts from the vicinity of Windsor, showing at one place in the valley "gravel which is unmistakably that of a kame"; just south, what "seems to be a kame deposit;" and 1½ miles south, "distinct remains of the kame," forming the east border of the terrace, both kame and terrace being 150 to 170 feet above the river. For the next 11 miles no indication of the kame are seen; and beyond are only remains at long intervals more or less distinct.

The preceding figure is part of a section, given on p. 45 of the Report, intended to show the general features of the southern part of this kame ridge (exaggerated relatively in height) in Hartland and Windsor; and the following (from p. 40) is a transverse section of the Connecticut valley through the Hart-

2.



Transverse Section in Hartland and Plainfield.

land deposits, exhibiting the position of the kame just west of the river channel, and its relation to the terrace-formation and the several terraces of the valley.

The adjoining figure, from page 37 of Upham's Report, will help further to explain the author's views. It represents the Hanover "kame," with the outline of the terrace-plains on the opposite sides of the Connecticut. The kame, *k*, is represented as constituting a ridge, coarsely stratified, buried beneath the terrace formation, up to its very top, on the landward side, but uncovered on the side toward the river. A section taken a little farther north would have exhibited the "kame" projecting above the terrace-plain.

3.

Section of the Hanover Kame, *k*, on the east side of the Connecticut River, *r*; *m n*, the till-covered underlying rocks; *a*, terrace in Norwich 505 feet high above mean-tide level and 132 feet above low water in the river; *b*, terrace in Hanover 515 to 545 feet high.

"Kames" are also described as occurring in the valley to the north, but at long intervals.

As to origin:

P. 176. The kames "were deposited, as explained on pages 13 and 14, by glacial rivers, at the final melting of the ice sheet, in *channels formed upon the surface of the ice.* When the bordering ice-walls and its separating ridges and masses disappeared, the gravel and sand remained in long steep ridges, or in irregular short ridges and mounds."

P. 44. The infrequency of angular fragments and bowlders shows "that the kame of the valley was formed in an open ice-channel." P. 14. On the ice in these "channels were deposited materials gathered by the streams from the melting glacier. By the low water of winter, layers of sand would be formed, and by the strong currents of summer, layers of gravel, often

very coarse, which would be very irregularly bedded." "The glacial rivers which we have described appear to have flowed in channels upon the surface of the ice, and the formation of the kames took place at or near their mouths, extending along the valley as fast as the ice-front retreated." P. 44. "When the river entered upon the work of excavating its present channel in the alluvium, the kame was a barrier which confined erosion to the area on one of its sides and protected its opposite side; so that this ridge of gravel often forms the escarpment of a high plain with the river flowing at its base."

The chief points urged by Mr. Upham with regard to the so-called "kames," exclusive of those pertaining to mode of origin, are:—origination: after the till and before the stratified drift of the terraces; material: chiefly beds of gravel; structure: usually arched or anticlinal; situation: generally between the river and the upper terrace, and often making the riverward limit of the latter, also, in many cases, partially isolated and ridge-like, owing to a depression between it and the terrace, and sometimes a large depression; height: frequently the same with that of the upper terrace or a little above it. Further, his descriptions show that he refers coarse cobble-stone deposits in the riverward part of the terraces always to "kames."

In my study of the facts relating to the Connecticut Valley "kames," I commenced at Windsor, the southern limit of the great line of "kames," and examined the valley formations at various places from that place to Lyme, and thence northward to Barnet and Lancaster: and the report I have to make is unfavorable to the "kames." I made levelings at various places in order more surely to identify the terraces mapped by Mr. Upham, and to apprehend their true relation to the Connecticut Valley, and also, to add, if possible to the facts. My trials soon satisfied me as to the essential correctness of his measurements.

Windsor.—At Windsor (on the west side of the Connecticut) the upper terrace of the village rises to a height of about 216 feet above the river or 520 feet above the sea-level. I saw no good opportunity for a satisfactory examination of the material of its lower part beneath the village; but in the upper part found it to be fine sand and loam, though somewhat pebbly through the upper 25 feet.

South of the village lies Ascutney Pond, a north and south body of water made by damming the waters of Ascutney brook; on the *east*, the pond is separated from the Connecticut River by a ridge of stratified material, nearly flat-topped, having about the same height as the upper terrace. Mr. Upham says, somewhat doubtingly, that this ridge "seems to be a kame de-

posit." It ends southward in rocky ledges. A mile and a half farther south, the high river terrace consists along its eastern margin of very coarse gravel, and is pronounced therefore to be in this part the "remains of the kame."

I found this ridge east of Ascutney Pond to consist mainly of loamy material, or sandy loam, like the terrace west of Windsor, with little gravel and that chiefly over its upper surface or in an upper layer. But directly *west* of the Pond there is a terrace (not referred to particularly by Mr. Upham) whose material is made up largely of coarse gravel, in part cobble stones, and coarsest in its upper layers, which in this portion is as much entitled to be called "kame" as that "a mile and a half farther south." This terrace rises westward to a level plain at 448 feet above the sea-level, and then another at 480, and this last rises to 525 feet, which is the height given by Upham for the possible "kame" east of the pond. Its gravelly character continues, but diminishes northward.

I found no evidence whatever that the eastern portion of the terrace was a "kame," that is, a part separate in time of origin from the rest; the evidence was all against such a conclusion. Moreover there was an abundant source at hand for the amount of coarse gravel and cobble stones; for Ascutney or Mill Brook, rising in northwest Reading, flows with rapid descent by the north side of the lofty Ascutney Mountain (3320 feet), and would have been a great transporter from the drift-covered country it drained. The position of the stream, and its relation to the southward-flowing Connecticut, account for the distribution of the "kame" material or coarse gravel of the Windsor region, including that of Windsor village, mentioned by Mr. Upham, and also for the isolation of the ridge on the east side of the pond.

Two miles north of Windsor a kame is entered on Mr. Upham's map. Much coarse gravel here makes the outer or westward portion of the upper terrace, which is by the map 500 feet above the sea-level. Besides coarseness of gravel, I saw no evidence of a kame, that is of any deposits that were distinct from the terrace in original deposition. A brook comes from the west just north of the "kame."

Hartland station, 4½m. north of Windsor.—At this place stands the "kame" ridge represented in Upham's section reproduced, on page 456, of which he says: "At one place, east of Hartland depot, this plain (that of the upper terrace) has been swept away from both sides, and the kame forms a conspicuous steep ridge 125 feet in height [above the depot plain, 240 feet above the river]. Wherever it is exposed, it is readily recognized by the pebbles which strew its surface, and which are very rarely found in the ordinary modified drift of the valley."

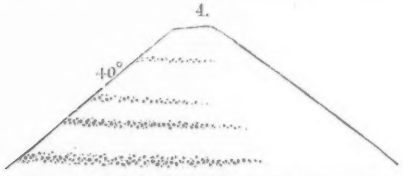
I ascended this prominent "kame" with my interest greatly augmented by the description in the Report. The narrow plain between it and the station (see the section) was covered with pebbles from an underlying gravelly layer. The same gravelly layer made apparently the base of the "kame," for some loose cobble-stones were found at the base of the slopes and for 10 to 15 feet above. But on ascending the ridge, no gravel was anywhere observed at a higher level; on the contrary, all was fine loam or fine sandy loam to the very top. And on descending, the same proved to be true; the only gravel was at its base, 50 feet above the river and nearly 200 feet from the top, according to my leveling. There were no good sections, but if made of gravelly layers, loose stones or pebbles would have worked out to the surface and shown themselves somewhere over the earthy sides.

A few rods west and northwest of the Hartland depot there was gravel in the terrace, and much of it; and according to the description of "kames," there was, as far as material goes, a "kame." On the first terrace-plain, about 65 feet (by my leveling) above the railroad track (or 486 to 490 above the sea level) large stones (1 to 10 inches across) lay over the surface, and very many in the sloping section of it facing the railroad track. From this terrace-plain, some rods to the west, there is an abrupt rise to the next higher terrace, and here the material is fine sandy loam with no pebbles. The natural conclusion is that the gravelly stratum is a lower part and the sandy loam an upper part of the same terrace formation, precisely as in the so-called "kame;" and, secondly and accordingly, that the "kame" is nothing but a piece of the terrace-formation. Lull's Brook here comes in from the west and is no doubt accountable for the coarse gravel.

North Hartland, nearly 4 miles north of Hartland.—At North Hartland station, there commences, according to Upham's map, another "kame" a mile long; it is near the river, close by the west side of the railroad. Its height by the map is that of the upper terrace-plain, or 550 to 560 feet above mean-tide level. Very coarse gravel shows itself in an oblique section of the terrace formation or "kame" facing the railroad, becoming cobble-stone layers 70 to 80 feet above the track. The coarseness diminishes to the northward. The large torrential stream, Quechee river, rising in the Green Mountains, enters the Connecticut here, and seemed to be a sufficient source for all the depositions; while the fact that the contributions were contributions to the Connecticut, which was in rapid flow off its mouth, accounted for the distribution of the especially coarse accumulations along the riverward border of the terrace.

In Hartford, Vt., at White River Junction, 4½ miles north of North Hartland.—On the west border of the Connecticut about White River junction, or at the mouth of White River, there is a short "kame" according to Upham's map south of this river, and one, a mile and a half long, north of it. The White River valley is here very broad, like a piece of the Connecticut, and as it rises westward but slowly, it opens to view a portion of the Green Mountain range, which is the chief source of its waters. The Connecticut valley terraces of the region are high—not far from 180 to 235 feet above the river, or 510 to 570 above mean tide level; but that to the north, owing to the retreat in the hills is much the most extensive, and hence the greater length of the northern of the two "kames."

The *southern* "*kame*" commences within a few yards of the railroad station and hotel, where an excellent section of it is exposed to view. The pitch of the slope toward the Connecticut is about 40°. The structure is well-bedded throughout. The layers consist of cobble-stones, finer gravel and coarse sand. The coarsest cobble-stone layers are below, and some of the rounded stones from them are one to over one and a half feet in diameter. Other cobble-stone layers, less coarse, occur at different levels above, alternating with an increasing thickness of gravel; and toward the top, which is near the top of a ter-



Upper part of the section of the "kame."

race-plain, the material is finer gravel and sand. Fig. 4 shows the position of the cobble-stone beds in the upper half of the section. The beds are not continued through the figure because in the western portion of the section the layers were mostly concealed by slides; but it was manifest from the few and smaller stones on the surface that there was a marked diminution in coarseness to the westward even in the first 100 yards.

The cobble-stone beds exposed to view in the section stop short off below at a level about 20 feet above the level of the railroad track, or 56 feet above the river (low water), and underneath occurs a bed of coarse sand, having the flow-and-plunge structure well marked. A section of the same sand-bed was observed 70 yards to the south, evincing that it is not a local deposit. But the depth to which it was exposed was hardly 8 feet; and it may be that there are other stony layers

underneath. Above the top of this section there is a nearly even terrace-plain, 160–170 feet above the river, or 493 to 503 above mean-tide level. This plain rises to the southwest to a maximum height (not observed by Upham) of 570 feet. The material is fine sand and sandy loam. But along the riverward border of this terrace plain, where it is lowest (493 feet), stands a steep narrow ridge, 50 to 65 feet high, which, judging from the stones of its surface, is made chiefly of beds of cobble-stone gravel. The top is 546 feet (Upham), above the sea.

The cobble-stone character of this ridge and its position make it eminently "kame"-like. But the evidence from the section described, as well as from the plain around, is directly opposed to the idea that it is the top of a buried gravel ridge, existing there before the terrace material was deposited.

In the section, the obvious facts are: that these upper cobble-stone beds—those of the top ridge—are underlaid, first by layers of sand and fine gravel, and then below by alternations of coarser beds; that all the beds are horizontal instead of arched; that they diminish rapidly in coarseness westward, or up White River, showing this even in the first 100 yards, and less rapidly southward or down the Connecticut, the coarsest deposits being at the angle in the terrace formation between the two streams. All the beds are evidently those of the terrace-formation, and the cobble-stone ridge at top is the youngest instead of the oldest.

The *northern "kame,"* or that north of White River, commences about half a mile from the railroad station. A section is exposed to view at its southwest angle, facing White River, exhibiting very similar features to those presented by the northern kame near the railroad. It is horizontally bedded throughout, and the coarsest beds are below; and some of the rounded stones from the beds are two feet in diameter.

But the cobble-stone beds are of less extent, for they reach only to a height of 45 feet above the railroad track, or 81 above the river, and are coarsest at 16 to 26 feet. Above the 45 feet the beds are of coarse and fine gravel, and increasingly finer to the top of the terrace, 510 feet (Upham) above mean tide. Below 15 feet above the railroad the beds are concealed.

On the top of the high terrace, along its riverward border, some spots of cobble-stone gravel occur, but no distinct gravel ridge like that of the southern kame.

The interior of this "kame" is fortunately more or less perfectly exposed to view in both *longitudinal* and *transverse sections*; and it is remarkable that these sections have nothing "kame"-like in them.

The longitudinal or *north and south* section extends along a

cut or gorge commencing close by the west side of the cobble-stone exposure just described. The gorge (with its carriage road at bottom), seemingly divides off a veritable "kame" from the terrace west of it; but the beds on the opposite sides of this cut so correspond, that there can be no doubt of stratigraphic unity.

The section of the "kame" along this gorge is more or less obscured by slides, but not in all parts. It shows, first, that the stony beds diminish rapidly in coarseness away from White river or to the north. One hundred feet up the gorge, the cobble-stones are half smaller and extend up to a height of only 30 feet above the level of the railroad, or 66 feet above the river, and beyond this they continue to diminish. At 400 feet up the gorge, the ascending road along its bottom reaches a height of 28 feet above the railroad level, and here, in the exposed section on the *east side*, there is a bottom layer of sand and above the sand 30 feet in thickness of clay; and this clay outcrops on the west side of the gorge as well as the "kame"-side, proving that the deposits of the supposed "kame" are one in bedding and material with those of the terrace formation, just as the high terrace plain above the whole (510–520 feet) is one from the Connecticut westward.

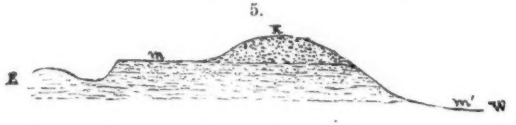
To the eastward of this section, toward the railroad, the deposits diminish in coarseness; and the same change continues northward along the railroad, where the surface material of the lower part of the terrace-slope shows stones only to a height of 20 or 30 feet, or less, above the track.

One of the *east-and-west* sections of the "kame" exists about half a mile north of the south end. A gorge intersects the deposit which is cut down to the level of the railroad track and extends inward (westward) to the center of the "kame" line. But there is nothing kame-like within it, and least of all at its inner extremity. On its *north* side, it has no cobble-stone beds, not even gravel beds; the material is fine sand delicately straticulate. On its south side, in the part nearest to the river, there occurs, in a large mass that has slipped down from above, a thin bed of small stones (three inches in diameter) with some gravelly and sandy layers below; elsewhere the material is sand. In the inner part of the cut, besides the fine sand, there is a bed of light-colored clay and sandy clay between 60 and 90 feet above the railroad, and above this within a few feet of the top, sand and fine gravel.

There is however one "kame"-like feature. Upon the top of the terrace (here about 510 feet above mean tide), near the inner end of the gorge there is an isolated knoll about 30 feet high, and of rounded form, which has many cobble stones over its surface, some of them 10 inches in diameter—indicative of

cobble-stone beds within. It has no continuation north or south. The material of the plain around is sand or fine gravel, like that of the upper part of the section. The following figure shows the position of the gravel-made knoll, the form of the

5.



surface north and south, and a section of the beds which according to the facts in the gorge, underlie it. The material of the knoll at top is manifestly the *latest* of the terrace-deposits.

The beds below the level of the railroad were not exposed to view at this place.

The second *east-and-west section* occurs about a fourth of a mile farther north. A long and deep gorge here cuts through the deposits of the 520-foot terrace, nearly to the level of the river, intersecting the "kame" line and extending nearly half a mile to the westward. There is less of kame-like features here than in the preceding gorge. Along the bottom of the deep cut, where a stream flows in some seasons, lay pebbles and some cobble-stones, derived from layers below the level of the railroad track, and these continued for about 300 yards west of the railroad. At a higher level the material is sand or very fine gravel, and the latter in some parts at the top. The sides of the cut were mostly covered by the fallen sands, so that the existence or absence of beds of clay could not be ascertained. A unity of structure from east to west was manifest. Nothing answered to the description of a kame; all was apparently of the terrace formation.

Hanover, New Hampshire, four miles north of White River Junction.—In the town of Hanover, a "kame," according to Upham's map, borders the Connecticut for three miles, to a point north where the river makes an abrupt bend, and thence it follows in the same direct line, the *western* or Vermont side of the river in the towns of Norwich and Thetford, nearly to Thetford village, making in all a length of about seven miles.

The only section of the Hanover "kame" which I have personally examined, is that on the road side between the bridge and the village—the one figured by Upham on page 39 of his Report. At this place, the riverward portion of the stratified drift, or that spoken of by Upham as the "kame," is separated from the following portion by a depression produced by undermining and a dropping of great masses to a lower level, and consequently there are at this place two bluffs, the western which is that of the so-called "kame," and an eastern, which is referred by Upham to the ordinary terrace-formation. In his

figure of the section of the kame here exposed to view, it is made to consist of somewhat arched beds, with alternations of coarse stony layers and finer material alike from top to bottom. I found the bedding horizontal, like that of the eastern of the bluffs; its beds, composed largely of sand and fine gravel, with but few of cobble-stones: and the top portion made of very fine sand, identical in its light color, fine straticulation and other features, with the top portion of the eastern bluff. The latter bluff differs in consisting throughout of stratified sand, and this difference between the deposits near the river and those more remote is not uncommon.

Prof. O. P. Hubbard, of the Medical School of Dartmouth College at Hanover, and formerly Professor of Chemistry and Geology in the Academic Department, has obtained for me the following additional facts respecting the region of the supposed "kame."

He states that no coarse gravel or cobble-stone beds exist along the top of the "kame" *south* of the above mentioned section for the half mile to Mink Brook, and none *north* of the same for nearly a mile, so that this kind of evidence as to the existence of a "kame," fails in these portions. Farther north, above the village of Hanover, there is on the "kame" ridge an area of cobble-stones, and two to three hundred yards beyond this, across a deep cut leading to the river, a grass-covered knoll made up of coarse gravel and cobble-stones, some of the stones a foot or more in diameter. The knoll was found by measurement to be fifteen feet high above the terrace-plain; it marks the spot which is made by Upham, the highest part of the kame, 556 feet above mean-tide level. Prof. Hubbard ascertained with a spade that the knoll was composed of coarse gravel, and *rested on* fine sand or sandy loam like that which makes the top portion of the terrace-formation between there and the village and also at the bluffs described above and elsewhere. He concluded, therefore, that the coarse cobble-stone deposit was but 15 feet thick; and, from the level of the other cobble-stone area, that the latter corresponded in position to the lower portion of this deposit. In the deep cut between the two cobble-stone areas the beds are not exposed, but no stones show themselves, and the material was evidently of the same fine sandy nature. Just south of the more southern area three large excavations have been made on the east side of the "kame" ridge to its top, for filling a bog, and these show only sand; but the northern is so near the cobble-stone layer that some of the stones have fallen into it. The evidence obtained by Prof. Hubbard thus appears to prove that the coarse gravel of the two areas is only the top deposit of the terrace-formation, such as characterizes in many other places its riverward portion.

Norwich, Vermont.—The continuation of the Hanover "kame" northward along the west border of the Connecticut in Norwich, passes, near the end of its second mile, the valley of Pompanoosuc River. About a mile south of this turbulent stream, a road ascends from the borders of the Connecticut River to the summit of the high terrace, crossing the "kame" where its height is 565 feet (Upham), half a mile south of the highest point, 600 feet. Along the road are sections of the deposits, showing the inner nature of the Norwich "kame." Where the road commences the ascent some cobble-stones lie scattered over the surface, such as had been found common along the road at the base of the "kame" for the half mile or more to the south. Above this, for the next hundred feet, there is sand, finely straticulate, with occasional fine gravel. Nearing the top, the beds become coarse gravelly, and then there are large cobble stones; and this upper coarse-gravel portion rises above the general level of the plain, making a low ridge which is the crest of the so-called "kame." In the higher part, to the north, some stones, as stated by Upham, are 4 to 5 feet in diameter and angular.

Nothing was observed on the ascent from the river, or on the west side, to suggest a suspicion that this cobble-stone deposit was the top of a narrow range of coarse gravel beds buried beneath the terrace-formation; on the contrary, the evidence from the sections along the ascent, and especially the succession of beds toward the top from sand beds to gravel beds, and then to the coarse cobble-stone gravel, strongly confirmed the natural inference that all was one consecutive series, with the cobble-stone deposit the uppermost and therefore the latest. West of the cobble-stone ridge, or the "kame," the terrace has great extent. The surface falls off immediately 40 feet, exposing the materials that lie beneath, and these are sand and fine gravel as on the east side.

The Pompanoosuc river was probably the chief source of this coarse material of the summit. To the southwest, about the village of Norwich, the terrace is quite stony over much of its surface from the contributions to the terrace of Blood Brook.

In Thetford the "kame" becomes very low before the village is reached.

The other reputed "kames" of the Connecticut River valley I have not particularly examined, But as the line from Windsor to Thetford is "the kame of the Connecticut valley," essentially "a continuous gravel ridge or kame, extending 24 miles," and is made, in Mr. Upham's work, the text for the description of "kames" in general, details from the other minor "kames" in the valley are not necessary for a right conclusion.

Conclusion.—The conclusion from the investigation is, as already indicated, the following: that the supposed "kames" are portions of the terrace-formation, with which they usually correspond approximately in height; and that their materials were the same in source with the rest of the stratified drift, and the beds the same in time of origin.

The gravelly character of the terrace-formation off the mouths of the tributaries of the Connecticut is often mentioned by Mr. Upham; and, if the above conclusion is right, the coarse material of the "kames" is to be explained on the same principle. The position of these coarsest deposits, near the borders of the flooded Connecticut, whether they make the lower or the upper beds, is a consequence of the rapid flow of the waters in this great stream, which drifted away much of the finer material within reach and left stones. The coarsest stone beds at the mouth of White River are located where the two streams—both great streams then—join, that is, where the great contributor of gravel and stones encountered the great distributor.

The deposit of gravel and stones in the upper portion of a terrace I have attributed to the violence of the flood when at its maximum stage. But in the region of the so-called "kames," from Windsor to Thetford and beyond, floating ice was probably needed for much of the transportation; and ice-floes would have been abundant at the time, when the glacier-ice was in rapid process of dissolution about the slopes of the Green Mountains—the range at the head of the principal tributaries in this part of the Connecticut valley. At the same time, the Connecticut, by its rapid flow along its eastern side at one time and its western at another, would have determined an accumulation of stony material along its borders, as a great river now produces accumulations on its banks different from those more distant. Here the floating ice with its burden of earth and stones would have been stranded as well as other transported materials. Moreover such deposits might have been raised ten feet or more above the plain adjoining, as now happens on large streams from modern floods. But there is no occasion to account for a cobble-stone deposit along the whole top of any of the so-called "kames;" for, only a small fraction of each has a crest of this kind; or any difference in structure from the ordinary terrace-formation, except that in some cases, near tributaries, they have more of coarse gravel below.

In Haverhill the angular stones and gravel, brought down the Ammonoosuc on ice-floes, made in one place a thick till-like deposit lying unconformably over the stratified drift and continued some distance down the riverward slope of the terrace. This is an exceptional case, due probably to the fact that the White Mountains, the source of the stream, are near by.

But the ridge-like feature of many of these coarse upper deposits, on the riverward part of the terrace-formation, that is, their standing up 15 to 60 feet above the level of the terrace around, and sometimes higher, is in part, if not chiefly, due to erosion. The Norwich stony deposit, on the top, south of the Pompanoosuc, has a large and broad depression west of it; and so has that south of White River Junction, that of Hanover, and others. Even the little knoll described on page 463 has its adjoining depressions, as shown in the figure there given, and the gulch descending from the southern one of these depressions may be a further consequence. The waters of rains, making rills or streamlets, easily remove the sand and fine gravel of the terrace-formation; but they make comparatively little impression on the beds of coarse gravel and cobble-stones, because of the size of the stones and often also their partial consolidation by iron oxide (limonite). Hence the waters which fall over the stony surface find a place of descent and wear away on either side; and with every new inch of descent gained there is a gain in fall and force, and a quickening of the work of erosion. The channel begun is deepened and widened, waters from the plain flowing in and helping in the removal: and thus broad channels like river-channels may form over wide plains, and deep gorges be cut through to their depths if a place of discharge is at hand. Besides, the river at the time of greatest height swept over the terrace plains with often 40 to 60 feet or more of depth, and large denudation in some parts would have been the consequence.

The above explanations have reference to those so-called "kames" examined by me in the Connecticut River valley. I make no sweeping application of them to those which have been described from other regions that I have not seen. It was my purpose to have studied, the past season, also the terraces of the Merrimack valley, but time failed me.

The gravel ridges of the vicinity of Andover, Massachusetts, first described by Prof. E. Hitchcock, and lately studied with care and designated "kames" by Prof. G. F. Wright, appear to represent a phenomenon of a different class. I had the guidance of Prof. Wright in a day's excursion over them, and was led to think, as he does, that these isolated ridges of unstratified coarse gravel and stones are of morainic sub-glacier origin; and, perhaps, lateral, though sub-glacier, moraines, left between bodies of ice that moved southeastward along the depressions—now marsh-filled—which exist either side of them. But without more study of them, and especially of their relation to the deposits of the Merrimac valley, I would not express a decided opinion on the question.

Nothing has here been said with regard to the "kettle-holes," that is, isolated kettle-shaped and often pond-containing depressions, which, in Mr. Upham's view, were connected in origin with the "kames;" and for the reason that they occur also over ordinary terrace-plains. Further, Mr. Upham's hypothesis as to the origin of "kames" there is obviously no occasion here to discuss.

Some points in the explanations above advanced need, in view of the difference of opinions among writers, further consideration, and will be made the subject of another communication.

ART. LX.—*Japanese Seismology;** by Professor C. G. ROCKWOOD, Jr., Princeton, N. J.

THE change in the foreign policy of the Japanese, by which that country was opened to the influences of western civilization, gave an impulse to several branches of scientific investigation for which Japan affords special facilities; but in no department has there been more hopeful progress than in the study of Seismology.

The opportunities for the development of this science in Japan are exceptionally good. Earthquakes are here quite frequent, averaging for the whole kingdom more than one every day, and sometimes far exceeding that number. Hattori has found native records of 817 shocks in the fourteen months from Nov. 1, 1854, to Dec. 31, 1855. The earthquakes also are mostly of moderate intensity and therefore better fitted for instrumental study than those violent and destructive convulsions which leave their record in ruined cities and decimated communities. The centers of learning and science, where are naturally gathered the greater number of persons qualified and disposed for such investigations, are on the shores of Yedo Bay, a district specially subject to earthquake shocks and whose geological character is tolerably well known. Here, in the capital Tokio, a society has been formed for the especial study of Seismology, including in its membership professors, both native and foreign, from the educational institutions of the city, having as its president a native Japanese, I. Z. Hattori, A.B. (Rutgers), and for its vice-president Professor John Milne; and which has printed, as the result of its first year's work, a volume of Transactions amounting to 188 octavo pages. Accounts of the work done in this society and contributions from its members on topics related to Seismology are also published from time to time in the Japan Gazette.

* Read before the Princeton Science Club, Oct. 27, 1881.

In directing attention to those who have labored in this field, we have to mention the names of E. Naumann, John Perry and W. E. Ayrton, I. Z. Hattori, W. S. Chaplin, E. Knipping, J. A. Ewing, G. Wagner, T. Gray and John Milne, all of whom have added to the available stores of information, by the examination of native records, or by the invention and improvement of instrumental appliances.

In the literature of Japan are found numerous accounts of past earthquakes, reaching back even to 295 B. C., at which time it is recorded "Fujiyama was upheaved." These native records have been examined by Dr. Naumann,* Mr. Hattori,† Mr. Knipping,‡ and Professor Milne,§ and have furnished abundant material for discussion. Indeed the amount of Japanese Seismological literature is unexpectedly large. Dr. Naumann mentions the titles of thirty-three and Hattori o thirty-four native books consulted in preparing their papers, while Milne is acquainted with sixty-five native earthquake books besides seven earthquake calendars. A part of this earthquake literature, especially the calendars, has a scientific value, but on the other hand much of it is made up of a series of anecdotes often of a trivial character. For illustration of these, a single one, selected from an account‖ of the great shock of 1707, will suffice.

"How an impetuous man fell down from up-stairs."

"Five or six young men were singing and drinking up-stairs in a tea house in Horiye. In the midst of their happiness they were suddenly alarmed by the earthquake and at once became bewildered. While one of them was looking out he missed his footing and fell down from the ladder into a konomono-oke (a cask containing radishes pickled in salt and bran which is very offensive to the nose). The others who were yet up-stairs intended to come down. But the man in the cask looking up said that below all was chaos and it would be better to remain up-stairs. The reason why the man below said that all was chaos was because he had not perceived that it was by accident that he had fallen into the cask."

The earthquakes contained in Naumann's and Hattori's lists have been discussed by their authors and by Ayrton,¶ with

* Ueber Erdbeben und Vulcanausbrüche in Japan. Mittheilungen der deutschen Gesellschaft für Natur- und Völkerkunde Ostasiens. 15tes Heft.

† Destructive Earthquakes in Japan. Transactions of Asiatic Society of Japan, vol. vi, p. 249.

‡ Verzeichniss von Erdbeben, wahrgenommen in Tokio, von Sept. 1872 bis Nov. 1877. Mittheilungen der deutschen Gesellschaft, etc., Ostasiens. 14tes Heft.

§ Japan Gazette, June, 1881.

‖ Milne in Japan Gazette, 1881.

¶ Note on the Periodicity of Earthquakes in Japan. Transac. Asiatic Soc. of Japan, vol. vi, p. 320.

respect to the seasons, the motions of sun and moon, the frequency of sun-spots, meteors, etc.; and Professor Chaplin * has examined in the same way the records for three years (1875–8) of the Palmieri instruments in the Meteorological Observatory of Tokio. But the results are entirely negative, not confirming Professor Alexis Perry's deductions from a similar examination of his lists, although Hattori and Ayrton both think they find some indications of a periodicity in destructive shocks.

Besides examining native records, much attention has been given to the instrumental investigation of the earth-motion. In this work Perry and Ayrton, Wagner, Chaplin, Ewing, Gray and Milne have all had a part.

The devices suggested by former observers have been here tested anew. Pendulums long and short, suspended and inverted, with bobs light and heavy, and making their records by scratching a smoked plate, by pushing light rods arranged against them, or by pulling cords and turning pointers over graduated arcs, the fluted mercury dish of Cacciatore, the graduated cylinders of Robert Mallet, and the bent tubes and loaded springs of Palmieri, as well as the microphone suggested by Rossi have all been employed and have done good service.

But no one of these was entirely satisfactory. Not to mention other difficulties, the pendulums and loaded springs had each a normal rate of vibration, and were ready to take up and accumulate earth vibrations of similar rate, while remaining to a considerable extent unaffected by those of a different period. So that the records of the earth-motion were complicated or perhaps entirely concealed by those due to the normal vibration of the apparatus. This difficulty, long known, was stated and mathematically discussed by Perry and Ayrton in a paper read before the Asiatic Society of Japan in 1877 and afterward published.† The remedy suggested by them was to support a heavy ball within an iron box, by spiral springs of such stiffness as to make its normal rate of vibration much quicker than any ordinary earthquake wave.

Moreover, while these instruments of former observers gave some more or less accurate indication of the time of an earthquake shock, and of its direction of propagation and relative intensity on some arbitrary scale, they afforded very little knowledge of the extent or character of the actual motion of an earth-particle, and to this end especially has tended the instrumental work of Japanese investigators. In this direction

* Examination of the Earthquakes recorded at the Meteorological Observatory of Tokio. Transac. Asiatic Soc. of Japan, vol. vi, part II.

† On a neglected principle that may be employed in Earthquake Measurements. London Phil. Mag., V, vol. viii, p. 30, July, 1879.

was the attempt by Dr. Verbeck in 1873 to support a heavy planed block of wood upon four crystal balls, these resting upon a polished marble slab carefully leveled. The block was then in neutral equilibrium and a pencil attached to it would leave a record of the motion upon a paper fastened to the slab beneath. Such a record was found to be too minute to be of service, and an important aim of later devices has been to procure in some way an *enlarged* record of the earth motion. This has been accomplished, in two ways: by employing an indicating lever with unequal arms, the shorter arm being acted on by the motion of the earth, while the longer arm carries the writing style which makes the record, or by causing the earth-motion, through the medium of a fine cord, to turn a small pulley to whose axis is attached a long light pointer.

The earliest apparatus by which a magnified record was obtained was Wagner's Pendulum Seismometer, first described in a paper read before the German Asiatic Society of Tokio in June, 1878, and printed in the Transactions of that Society. After two years experience a full description of the apparatus was published in the Japan Gazette (July 10, 1880).* It consists of an iron ball weighing forty or fifty pounds, suspended by a bundle of silk threads three feet long. At the moment of a shock this heavy ball by its inertia remains stationary. Beneath the lowest point of the ball, a light vertical indicating lever or pendulum is supported by a bar rigidly connected with the earth. The fulcrum of this indicating lever is formed by a metallic sphere $\frac{3}{8}$ inch in diameter, on which it rests by a smooth plate forming the top of a short hollow cylinder of the same internal diameter as the metallic sphere. The point about which this lever pivots is therefore the center of this small supporting sphere. The upper end of this lever, the shorter arm, engages with a similar small sphere attached to the lower part of the heavy iron ball; while the lower and longer arm is attached to a light thread that passes through a hole in a porcelain plate. Of course any motion of the ground is transmitted to the support of the indicating pendulum and causes relatively magnified motion of the lower end of the same the amount of which is indicated by the length of thread drawn through the hole. It appears to the present writer that the elasticity of the silk cords supporting the heavy ball would introduce an element of uncertainty into the indications of this apparatus, as quantitative results could be hoped for only on the assumption that there was no *vertical* motion of the heavy ball with respect to the support of the indicating pendulum. Of course this seismometer gives indication of the amount of horizontal motion only. The *direction* must be obtained from other

* Transactions Seismological Society of Japan, vol. i, part I, p. 54.

apparatus used in connection with this, as must also the vertical component.

Another device for obtaining a magnified record of the earth-motion is Gray's Rolling Sphere.* This consists of a heavy lead or iron sphere resting in neutral equilibrium upon a level plane, and therefore free to roll in any direction. Above the sphere an indicating lever is supported in a vertical position, by a sort of spring universal joint, so that its lower extremity, the shorter arm of the lever, engages with a hole in the highest part of the sphere, while its upper and longer arm carries the recording style. The method of arranging the fulcrum of this lever is peculiar. The light rod forming the lever passes centrally through a small disk to which it is fastened. This disk plays within a horizontal ring, from which it is supported, through the medium of four bent springs, which are attached by one end to symmetrical points on the ring and by the other to the edge of the disk. The lever has a small weight on its lower arm sufficient to bring the center of gravity below the fulcrum and to make its normal rate of vibration slower than that of the earthquake. The lever supported in this way is, by the elasticity of the springs, free to move in any direction as influenced by the motion of the heavy sphere.

Gray's Double Bracket Seismograph † also gives a magnified record of the actual motion of an earth-particle. This consists of a post planted firmly in the ground, to which is hinged, by its longer side, a light but strong frame, something like a gate, measuring 60 × 15 centimeters. The upper hinge is a knife edge in a ring, while the lower is a point resting in a conical socket. To the outer edge of this frame is hinged in the same way another similar but somewhat lighter one, loaded on *its* outer part by a thick metal disk of considerable weight, which by virtue of its inertia forms the stationary point of the seismograph. When ready for use the planes of these two brackets are placed at right angles to each other, and each makes an angle of forty-five degrees with the face of the post. The record is made through the medium of an indicating lever similar to that above described with the rolling sphere, and supported below the center of the heavy disk by an arm extending out from the post.

Gray's Pendulum ‡ Seismometer aims to record the earthquake motion by means of its components in *three* directions at angles of 120°. It consists of a heavy weight hanging by a cord three feet long, from the middle of a stretched wire. It is

* On Instruments for Measuring and Recording Earthquake Motions. London Phil. Mag., V, vol. xii, p. 199, Sept., 1881.

† London Phil. Mag., V, vol. xii, Sept., 1881.

‡ Transactions of Seismolog. Soc. of Japan, vol. i, part I, p. 44; also Lond. Phil. Mag., V, vol. xii, Sept., 1881.

thus able to move in a vertical as well as a horizontal direction, and the amount of this vertical motion is recorded by attaching to the upper part of the pendulum a fine thread which turns a small pulley above and thereby moves a long pointer. To register the horizontal motion, three radiating cords pass from the center of inertia of the heavy bob of this pendulum to three horizontal pulleys to which are attached long pointers that magnify the actual motion twenty-five times. The method of attaching these pointers to the pulleys is new and ingenious. The pointer is hung to the under side of the pulley by a bifilar suspension, so that there is no tendency for the inertia of the pointer to carry the pulley too far, as was found to be the case if they were rigidly attached. The inventor proposes to prevent the pendulum from accumulating earth vibrations that may happen to synchronize with its own normal rate, by allowing a pointed rod to slide on a glass plate below the bob and weighting this sufficiently to produce the necessary friction.

In Ewing's* Pendulum Seismograph this same object is accomplished by using a pendulum twenty-one feet long, so that its normal time of vibration is about five seconds, much longer than any earthquake vibration. The bob of this pendulum, which has been erected in the University of Tokio, is a cast iron ring, whose section is 2×4 inches and internal diameter $16\frac{1}{2}$ inches, and which is suspended in a horizontal position from the top of a firmly braced framework. This heavy ring is crossed by a diametral bar, at the middle of which are applied the short arms of two bent levers, whose long arms mark upon circular smoked glass plates, caused to revolve continuously by clockwork. The planes of these two bent levers are placed at right angles to each other, and they are supported by gimbal joints in such a way that each is affected by motion in one direction only. The horizontal motion of the pendulum bob is thus separated into two rectangular components which are recorded separately.

Another device which records the two components of the earth motion separately is Gray's† Rolling Cylinder Seismograph. Here a pair of exactly similar hollow cylinders of metal are placed on a smooth level plane, with their axes horizontal and at right angles. Being thus in neutral equilibrium they are free to roll, and their motions are recorded by the magnifying levers whose fulcrums are upon a fixed support above the cylinders and whose long arms write upon a moving drum or plate.

Still another arrangement, the Bracket Ring Seismograph, which has already done good service, is a modification of Zöll-

*A new form of Pendulum Seismograph. Transactions Seismolog. Soc. of Japan, vol. i, part I, p. 38.

† London Phil. Mag., V, vol. xii, Sept., 1881.

ner's horizontal pendulum, due originally to Chaplin and improved by Ewing and by Gray. It consists essentially of a weight supported on a horizontal bar, which is attached at one end to a vertical axis and at the other end carries a long pointer writing upon a moving plate. This will of course record only one component of the motion, viz: that at right angles to the direction of the pointer, and such apparatus must be used in pairs placed at right angles to each other.

The apparatus for vertical motion, which was used in connection with this, was a vessel of water supported from above and having a flexible bottom, which would be acted upon by the inertia of the liquid and would make its record by a multiplying lever upon a moving plate.

A modification of the conical pendulum by Gray * promises to afford a very sensitive seismograph but it cannot well be described without diagrams.

Milne's tremor indicators † are also most delicate and sensitive. From a rigid frame is suspended, by a short wire, a heavy mass, against the sides of which rest two small horizontal bars of wood. Under the outer end of each bar a small mirror is hung by a bifilar suspension, one thread to the bar and the other to an adjacent fixed point. Then any motion of the heavy mass relative to its support causes motion of the bars, and so of a beam of light reflected from the mirrors. A motion of $\frac{1}{10000}$ of an inch is readily detected in this way.

Numerous other devices are described in the Transactions of the Seismological Society of Japan and in the other papers above referred to, and to these sources the reader is referred for further information in regard to them.

We are now to consider some results obtained by the use of instruments and the discussion of their records. The published volume of Transactions of the Seismological Society, Part II, contains from the pen of John Milne, a long account, amounting to over one hundred pages, of the earthquake of February 22, 1880. It is based on one hundred and twenty written communications received by the author, of which thirty were detailed replies to a series of printed questions. Our limits forbid anything more than a brief statement of a few selected points.

The direction of the shock was deduced from personal reports and from the indications of Palmieri's instrument, a Cacciatore and a pendulum recording its motion on a smoked glass. The general result was that there had been *two* shocks, the first in a direction approximately to or from N.N.W., the

* London Phil. Mag., l. c.

† See paper "On Recent Earthquake Investigations," by T. Gray in Chrysanthemum, vol. i, No. 5, May, 1881.

second N.N.E. or N.E. It is interesting to note that the pendulum records of the Luzon* earthquake of July, 1880, show likewise the presence of several wave directions in azimuths not widely different from those here stated.

Again, not only were many chimneys and similar objects overturned, but in numerous instances chimneys and monuments in the cemeteries were twisted upon their bases, sometimes through an angle of 20° or 30°, without being overthrown. The rotation was usually but not invariably in a direction contrary to that of the hands of a watch. As to the cause of such rotation, Mallet's explanation, which attributes it to the vertical through the center of gravity not coinciding with the center of friction, is rejected as not in accord with the great preponderance of rotation in one direction, and another explanation suggested by T. Gray is offered, to this effect: If any columnar object having a rectangular base is acted upon by a force parallel to either side or to either diagonal of the rectangle, it will tend to overturn without rotation. If, however, the force has any direction other than these, there will be a tendency to rotation in a direction determined by the relation of the line of force to that diagonal which lies nearest to it. If the rectangle be divided into eight equal triangles by the two diagonals and two medial lines parallel to the sides, and the alternate triangles be shaded, it will be seen that the rotation will be in one or the other direction, according as the direction of the force, falls in a shaded or an unshaded triangle. The direction in which a stone is found to have been twisted will then enable us to assign limits to the direction from which the impulse that moved it must have come, and will thus serve to indicate the direction of the earthquake shock. With regard to the earthquake in question, the direction inferred in this way from the numerous twisted grave-stones, agrees in general with the instrumental indications noted above.

This earthquake was sensibly felt over an area included within a radius of one hundred and twenty miles. From the directions of the shock as observed at Tokio and Yokohama, and from other considerations, the author concludes that the probable origin of this earthquake was nearly equidistant from Tokio and Yokohama, but somewhat to the east of them, under the eastern shore of Yedo bay. Indeed many of the recent earthquakes in Japan seem to come from that region. The geological characteristic of that district is beds of volcanic tufa and breccia very much faulted and contorted in the southern part and giving evidence of recent elevation. It is in the prolongation of a long line of volcanoes and volcanic islands, extending 1,500 miles southward into the Pacific through the Bonins; and it is also on another line of volcanoes, 3,000 miles

* This Journal, III, vol. xxi, p. 52, January, 1881.

long, extending from Kamschatka to the Philippines. The suggestion is ventured then that this and other earthquakes are to be attributed to action taking place about the end of the fissure in the earth's crust, marked by the first mentioned line of volcanoes, of which Ooshima, sixty miles south, is the nearest active vent; "that this line is still endeavoring to open for itself vents still farther north;" and "that beneath Yedo bay there is a point where volcanic agencies are endeavoring to force a way."

These conclusions as to the probable origin of the frequent earthquakes are further confirmed by later observations of two different sorts.* Prof. Milne in Tokio, and Mr. W. H. Talbot in Yokohama, have made careful time observations, using clocks with sensitive apparatus to stop them at the instant of a shock and keeping the clocks regulated by daily telegraphic comparisons. The result is that the shocks are usually felt in Yokohama from fifteen to thirty seconds earlier than in Tokio, indicating an origin nearer to the former place. Again seismometers were placed at Tokio, at Yokohama and at Kisaradzu on the opposite side of the bay, with special reference to determining the direction of the shocks, and gave the following results. On Jan. 7, 1881, the directions intersect, within *two* miles of Yokohama; on Jan. 22, 1881, the intersection was *four* miles south-southeast of Yokohama; and on Jan. 24, 1881, the intersection was *seven* miles south-southwest of the same place,—again all indicating an origin near Yokohama.

But perhaps the most interesting of recent results was obtained from the earthquake of March 8, 1881, some notes† on which were read before the Seismological Society on March 23d, by Prof. Milne. At this shock a complete record of the earth-motion for over twenty-five seconds was secured. The instruments used were a pair of "bracket ring" seismographs, writing upon a slip of smoked glass, for the two horizontal components, and a water vessel with flexible bottom for the vertical component. The bracket-ring machines (No. 1 and No. 2), were purposely placed so as to record vibrations at right angles to and in the direction of a line joining Tokio and Yokohama (S. 23° W).

No. 1 showed a decided motion, there being about seven vibrations in five seconds, or one complete vibration in $\frac{5}{7}$ of a second. The greatest indicated motion in this direction is about 1·3 millimeters.

No. 2 indicated very slight but sensible motion.

No. 3 for vertical motion showed about six distinct waves in a space indicating twenty-five seconds of time.

These records, confirmed as they are by the register of Palmieri's instrument and of eleven different pendulums, show

* Japan Gazette, Feb. 5, 1881. † Japan Gazette, April 2, 1881.

clearly that the main vibration in the vicinity of Tokio was in a general east-and-west direction. The time observations, and other considerations also, indicate that the origin of the shock was in the faulted region near Yokohama. Hence Milne is led to the conclusion that the vibrations observed were *transverse* to the direction in which the wave was moving, instead of normal as usually supposed; and that the wave, at least by the time it reached Tokio, was one of distortion, not of compression. It is probable that in any ordinary earthquake both sorts of wave are coëxistent, at least near its origin; but experiments made by Milne upon artificial shocks produced by the fall of a heavy weight, tend to show that the transverse vibrations are the more persistent and are felt to a greater distance than the longitudinal. Moreover, if the earthquake wave originated by the tearing open of a fissure in the rock and the sliding of the surfaces upon each other, a shearing force would be exerted which *might* produce a wave of distortion without any accompanying wave of compression.

The Japan Gazette of July 23, 1881, contains a note of some interesting observations on an earthquake of July 5, 1881, showing that the motion of the ground varied considerably in *direction* during the same shock. The records were made by Gray's Double-bracket Seismograph writing upon a smoked plate. Prof. Milne says:

"Near to the commencement of the shock the motion was N. 112° E. One and a half seconds after this the direction of motion appears to have been N. 50° E. In three-fourths of a second more it gradually changed to a direction N. 145° E.; and after a similar interval to N. 62° E. Half a second after this it was N. 132° E., and four seconds later the motion was again in the original direction, viz., N. 112° E. There appear to have been at some portions of the shock not more than four vibrations per second, at other portions there may have been as many as ten. The greatest amplitude of motion does not appear to have reached one millimeter."

The records of the various instruments agree in the indication that the amplitude of vibration of an earth-particle, at least in such shocks as ordinarily occur in Japan, is much smaller than has generally been supposed, not more than a very few millimeters. Of twenty earthquake shocks observed by E. Knipping* with Dr. Wagner's apparatus only two exceeded 2·5 mm. in amplitude, and a similar fact has been incidentally mentioned in respect to several of the earthquake shocks spoken of above.

To conclude our review of what has been done in Japan in this department of research, the results achieved can perhaps best be summed up in the words of Professor Milne himself in

* Transactions Seismolog. Soc. of Japan, vol. i, part I, p. 71.

his report to the British Association at its recent meeting in York, where he states them thus:—

"1st. The actual back-and-forth motion of the ground is seldom more than a few millimeters (usually not equal to one millimeter) even though chimneys have fallen.

"2d. The motion usually commences gently but is very irregular.

"3d. The number of vibrations per second usually varies between three and six.

"4th. During one shock the *direction* may be irregular.

"5th. East and west vibrations as recorded at Yedo (Tokio) have in some cases been shown by time observations to have traveled up from the south.

"6th. Many of the shocks which visit Yedo appear to have come from a district which is much faulted, and which gives evidence of very recent elevation."

This brief and no doubt incomplete survey of the field considered gives reason to believe that the knowledge of the phenomena and causes of earthquakes has received and will receive important additions through the labors of these residents of the far east; and that this youngest of the scientific societies of Japan, whose exhibition of seismographical instruments attracted 2,000 visitors in one day, has such a hold upon the interest of that community that it will not be left without support even though all its foreign members should be withdrawn from the country.

In conclusion I desire to say that for much of the information embodied in this paper I am indebted to the kindness of Prof. John Milne of the Imperial College of Engineering in Tokio.

NOTE.—Since this paper was written, the November number of the London Philosophical Magazine has come to hand, containing an article of 22 pages by John Milne and Thomas Gray, on "Earthquake Observations and Experiments in Japan." It is a *résumé* of work done by the authors during their residence there, and consists of two parts, the first devoted to a description of the instruments used, the second to a discussion of the Earthquake motion. The instruments are described under the head of 1. Seismoscopes; 2. Seismometers and Seismographs; 3. Instruments for vertical motion; 4. Apparatus on which to record earthquake motions; 5. Time-takers. In the second part, the authors discuss the relation of the normal and transverse vibrations, the details of the movement as illustrated by a copy of the instrumental record made by a pair of conical pendulums on July 25th, 1881, the relative frequency of earthquakes at different seasons, the effect on buildings, and the rotation of bodies. C. G. R.

Princeton, Nov. 17, 1881.

Art. LXI.—*An Apparatus for the Distillation of Mercury in Vacuo*; by Arthur W. Wright.

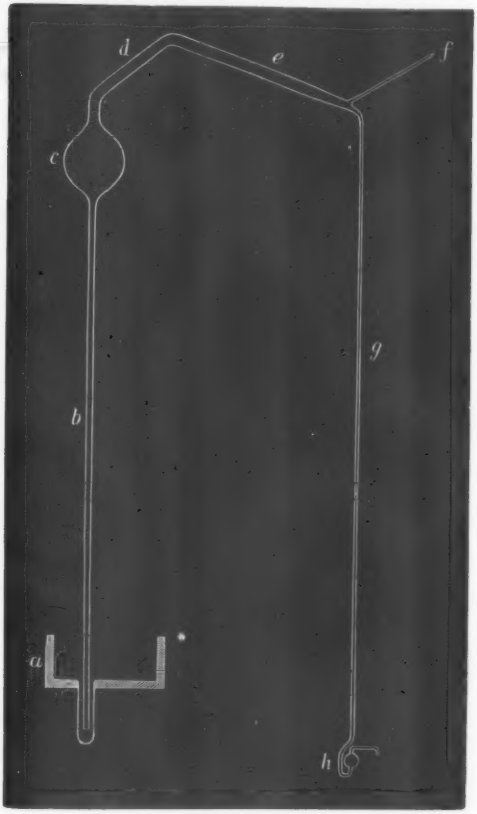
The importance of pure mercury in many of the operations in the laboratory makes a simple and efficient means of freeing from its impurities the ordinary commercial metal, or that which has become fouled by use, an object greatly to be desired. The familiar chemical methods, aside from their inconvenience, are not entirely satisfactory, and often leave the condition of the product uncertain. Distillation in the usual way, in retorts open to the air does not prevent contamination by oxidation, and the purity of the metal is further endangered by the liability to spurting and the possible presence of substances volatile at the boiling point of mercury. When the process is conducted in a vacuum, however, these drawbacks are avoided, and a perfectly pure product is obtained.

A very elaborate and complete apparatus for this purpose has been devised by Professor Weinhold,* which fully satisfies all the conditions of the problem. This instrument has provisions for the maintenance of the vacuum by means of a Sprengel pump which constitutes a special part of it, with suitable arrangements for adjustment of the mercury supply, the heat from the gas burner, and the like. The devices for securing these objects, however, render the apparatus somewhat bulky, and complicated in structure. A far simpler construction has been employed by Dr. L. Weber,† which however has no contrivance for maintaining or renewing the exhaustion, except by refilling with mercury, and otherwise leaves much to be desired. Its consists essentially of a long glass tube bent into a U-shape so that when filled with mercury and inverted with the ends of the tubes in vessels containing mercury it forms a double barometer, the bend of which is above the level of the metal and therefore vacuous. An enlargement at one side where the heat is applied by a small Bunsen flame gives an increased surface of evaporation. The mercury vapor condenses in the upper portion of the empty space and flows out through the other branch of the tube.

The apparatus devised by the writer is based upon Weber's plan of a double barometer tube, but with important modifications which secure substantially the advantages of the more complicated system of Weinhold. The most essential portions of it are represented, in section, in the accompanying sketch, which is drawn to a scale of one-tenth that of the instrument

* Carl's Repertorium für Physik, vol. xv, p. 1. † Ibid., vol. xv, p. 52.

itself. The principal member of the still consists of a single continuous piece of glass work, which, for convenience of description, may be regarded as made up of several distinct parts designated by the letters *b*, *c*, *d*, *e*, *f*, *g*, *h*.



The portion *b* is a straight, rather heavy piece of tubing, of about one centimeter exterior, and five or six millimeters interior, diameter. Its length is a little more than 76 centimeters. It is open at the lower extremity, and at the other is enlarged to an oval bulb, *c*, about 85 mm. in diameter and 120 mm. long. At the upper end of this is joined the portion *d*, *e*, having an interior caliber of about 15 mm. The vertical portion next to *c* is 25 mm., the inclined portion, *d*, 130 mm., and the sloping part, *e*, 300 mm. in length. The object in making *d* so long and giving it the inclined position was to

prevent any globules of mercury thrown up from the bulb entering the portion *e*. But it might well be somewhat shorter, as with proper care in the application of the heat no shocks of the mercury in boiling ever occur.

Toward the end of *e* the glass is narrowed, and, at the angle, it passes to a continuation *g*, which is a straight, vertical tube having an interior diameter of about one millimeter. The angle is so formed that the globules of mercury running down from *e* fall freely into *g* without accumulation at any point. This part of the apparatus is in fact a Sprengel pump, and the mercury as it passes out maintains the exhaustion of the whole tube at a very high point. A small tube, *f*, serves to make connection with the air-pump at the beginning of the operations. The tube *g*, at its lower end, *h*, is bent upward and a small bulb blown upon it, sufficiently large to hold enough mercury to fill *g* itself. Above the bulb the tube is bent into a horizontal direction, this part being 30 or 40 millimeters long, and then directly downward, forming the outlet for the mercury. The total length of *g* is 90 centimeters.

A cistern, *a*, serves for the reception of the metal to be operated upon. It is a wooden box 150 mm. square, and about 60 mm. deep. The joints are carefully fitted and the wood oiled and then well varnished, being thus rendered quite impervious. A small well, 80 mm. deep, for the reception of the end of the main tube, is made by inserting a thick glass tube in the bottom of the box. This arrangement, with the large area of the cistern, increases the range of adjustment of the latter, and makes it possible for several kilograms of mercury to pass through the apparatus before any such alteration of level in *c* is produced as to require a new supply, or a readjustment.

The glass tube and cistern are mounted upon a light wooden frame, the weight of the former with its contents being chiefly sustained by an iron ring which touches the bulb some distance below its widest part. Several layers of fine wire gauze carefully fitted to the lower half of the bulb are interposed between it and the ring, forming an elastic bed, and serving also to distribute the heat. A cylinder of thin sheet copper just large enough to slip through the ring is supported upon the latter by a narrow flange at the top. It extends downward about 60 mm. and is pierced with a number of holes just beneath the ring. It serves to direct the heated current from the burner upon the bulb, as also to protect the flame from movements of the air, and render it steady in its action.

The heating apparatus consists of a hollow ring having upon the upper surface 12 holes 2·5 mm. in diameter, spaced uniformly in a circle of 25 mm. radius, and concentric with the

glass tube which passes through it. The ring is fitted to the top of a common Bunsen burner, and the whole moves upon a vertical slide, with a clamp screw, by which it may be fixed at any point desired. It has been found advantageous in practice to use but six of the openings, and these all upon one side. A conical hood of sheet copper encloses the upper half of the bulb, and is prolonged by a tube of the same material, which covers the glass tube as far as the angle above *d*. The lower edge of the hood is at nearly the same level as the top of the cylinder above mentioned, and is about six centimeters wider than this, so as to project laterally some three centimeters all round. The upper portion of the copper cone and tube are wide enough to leave an interval of five or six millimeters between them and the glass. The heated gases from the holes in the cylinder, streaming through this space, envelop the bulb *c* and tube *d*, thus preventing condensation of the mercury vapor before it reaches *e*.

The cistern is mounted upon a vertical slide with a clamp screw, and can be moved up or down, the range of motion being about six centimeters. This makes it possible always to bring the mercury to the proper height within the bulb, and to suit the adjustment to the varying atmospheric pressure.

The apparatus is put in operation as follows: Connection having been established with a Sprengel air-pump by means of the tube *f*, mercury is poured into the cistern so as to cover the bottom of it to the depth of a centimeter or two. If pure mercury is at hand the bulb at *h* may be filled with it, if not the extremity of the outlet tube is sealed or otherwise tightly stopped. As the exhaustion proceeds the mercury rises in *b*, finally reaching *c*, and if all the air were removed, it would stop at the barometric height above the surface in the cistern. The latter is adjusted so that the top of the column is a little below the center of the bulb, *c*. When no more air can be with drawn by the pump, *f* is sealed with a gas flame and the connection with the pump severed. The apparatus is thus exhausted once for all, as subsequently it maintains the vacuum by its own operation. The burner, previously set some distance below the bulb, is now lighted and the flame made very small at first. The mercury soon becomes heated, vapor is formed, and after a time drops begin to fall from the interior surface of the bulb and tube above it. The flame is slowly increased and raised, until, in fifteen or twenty minutes, the vapor passes the angle at the top and begins to condense in *e*. As the globules of mercury fall into *g* they carry with them the residue of the air, gradually filling the bulb at *h*, and later the tube *g* itself. The point of the tube at *h* is now unsealed or broken off, and the mercury issues drop by drop into a vessel placed to receive it.

The operation now proceeds continuously, and the apparatus requires scarcely any attention, further than to keep the cistern properly supplied with mercury, and to remove the pure metal when necessary. The residual air is quickly removed from the tubes by the pumping effect in *g*, and after a short time each drop falls with a sharp click in the tube. The construction of the part *h* makes it easy to obtain pure mercury from the very beginning of the operation, an advantage not furnished by the other forms of the apparatus mentioned.

In adjusting the height of the cistern, *a*, allowance must be made for the tension of the mercury vapor in the upper portion of the tube. The cooling effect of the condensing tube, *e*, is such that this is usually from four to six millimeters, and it rarely or never exceeds one centimeter. The temperature of vaporization corresponding to the latter tension is less than 180°, as, according to Regnault's results, this is the temperature at which the vapor has a tension of eleven millimeters. The low temperature is of itself a matter of importance, both as regards economy in the application of the heat, and as diminishing the probability of volatilization of any substances which the mercury may contain as impurities.

The apparatus here described, when in use, consumes from one-third to one-half the amount of gas required for an ordinary Bunsen burner. The mercury does not come into active ebullition, but vaporizes quietly and entirely without shocks. The rate of distillation varies of course with the heat applied, but is from four hundred to four hundred and fifty grams per hour. After the burner is once adjusted the apparatus requires no attention and may be left to itself for hours, care being taken that the cistern contains sufficient mercury. When out of use the tubes are left with the mercury in them, remaining thus exhausted and ready for use at any time.

As the mercury in the bulb and the tube *b* retains all the impurities left behind in the process of distillation, these may at length accumulate in such quantity as to interfere with the proper operation of the apparatus, and to necessitate their removal. This is not likely to occur for a long time unless the mercury used is excessively impure. But when the removal is indispensable it may be effected either by opening *f*, allowing the mercury to descend into the cistern and thus be withdrawn, then refilling and exhausting as at first; or more simply by lowering the cistern until the mercury sinks below the bulb, in which case all but the small portion contained in *b* will run out into the cistern and can be drawn off by a siphon or otherwise, care being observed that it is not carried so low as to allow of the admission of air at the bottom of the tube.

The apparatus in operation has proved entirely satisfactory

in every respect, and extended use of it in the laboratory has suggested no modification. As mounted upon its frame it has a height of about 125 centimeters, and the base covers a space forty-five centimeters long and thirty-three wide. It is so light that it may readily be lifted and carried with one hand. The glass work was very skillfully constructed, after the design of the writer, by Mr. W. Baetz, of 96 Fulton street, New York City.

Yale College, Nov. 14, 1881.

SCIENTIFIC INTELLIGENCE.

I. Physics and Astronomy.

1. *Dynamo-Electric Machines.*—Sir W. Thomson concludes from a simple mathematical analysis of the currents in a dynamo-electric machine, giving a continuous current, that the formula $E = \sqrt{RR'}$ holds; in which E is the resistance of the exterior circuit and RR′ are the resistances of the field magnets and the revolving bobbins. If r represents the ratio of the total work to the lost work and $\varepsilon = \frac{R'}{R}$ the formula $r = 1 + 2\sqrt{\varepsilon}$ results. The case considered is that of a dynamo-electric machine provided with a shunt circuit.—*Comptes Rendus*, No. 12, September, 1881, p. 474. J. T.

2. *Rotation of plane of Polarization of Light by the Earth's Magnetism.*—M. Henri Becquerel states as the result of his experiments that the rays D traversing horizontally a column of sulphide of carbon of 1^m in length in a direction parallel to the magnetic needle, undergo at the temperature of 0° C. under the influence of the earth's magnetism at Paris, a magnetic rotation of 0′·8697. The direction of this rotation is from right to left for an observer reclining horizontally with his head toward the north. This number constitutes a natural constant by which we can convert into absolute measure the determinations of the magnetic rotations of the plane of polarization of light, and by which we can express the intensity of a magnetic field in terms of the rotation to which it gives rise. In the C. G. S. system, the above result is expressed by $1{\cdot}31 \times 10^{-5}$ which denotes the magnetic rotation of the D lines in a magnetic field of strength unity, between two points at a distance of unity.—*Comptes Rendus*, No. 12, September, 1881, p. 481. J. T.

3. *The value of the Ohm.*—Lord Rayleigh and Schuster have redetermined the ohm by means of the original apparatus used by the Committee of the British Association and have obtained the value $0{,}9893 \frac{\text{earth quadrant}}{\text{sec.}}$.—*Proc. Roy. Soc.*, xcii, pp. 104–141, 1881. J. T.

4. *Ephemeris of the Satellites of Mars.*—The following tables give a portion of the ephemeris, calculated by Professor H. S. PRITCHETT, including opposition time and the time of nearest approach. Table I gives the times of east and west elongation for Deimos, that for Dec. 13 and the alternate below being *West*, and the others *East*; table II gives the times of west elongation for Phobos. The effect of aberration (not included) would make the satellites about five minutes late at each elongation.

I. DEIMOS.

Date.	Wash. M. T.		Pos. Ang.	Dist.	Date.	Wash. M. T.		Pos. Ang.	Dist.	Date.	Wash. M. T.		Pos. Ang.	Dist.
Dec.	h.	m.	°	″	Dec.	h.	m.	°	″	Dec.	h.	m.	°	″
13	21	19	249·7	53·2	20	19	47	-----	----	27	18	12	-----	----
14	12	27	-----	----	21	10	54	-----	----	28	9	20	-----	----
15	3	35	-----	----	22	2	2	-----	----	29	0	28	-----	----
15	18	43	-----	----	22	17	10	-----	----	29	15	36	-----	----
16	9	51	-----	----	23	8	19	-----	----	30	6	44	-----	----
17	0	59	-----	----	23	23	26	-----	----	30	21	52	-----	----
17	16	7	-----	----	24	14	33	-----	----	31	13	0	-----	----
18	7	15	-----	----	25	5	41	-----	----	Jan.				
18	22	23	-----	----	25	20	49	-----	----	1	4	7	-----	----
19	13	31	-----	----	26	11	57	-----	----	1	19	15	245·9	52·4
20	4	39	248·5	53·7	27	3	5	-----	----					

II. PHOBOS.

Date.	Wash. M. T.		Pos. Ang.	Dist.	Date.	Wash. M. T.		Pos. Ang.	Dist.	Date.	Wash. M. T.		Pos. Ang.	Dist.
Dec.	h.	m.	°	″	Dec.	h.	m.	°	″	Dec.	h.	m.	°	″
13	5	31	-----	----	19	22	12	-----	----	26	14	53	-----	----
13	13	10	-----	----	20	5	51	248·5	21·5	26	22	32	-----	----
13	20	49	249·7	21·3	20	13	30	-----	----	27	6	11	-----	----
14	3	28	-----	----	20	21	9	-----	----	27	13	50	-----	----
14	12	7	-----	----	21	4	48	-----	----	27	21	29	-----	----
14	19	46	-----	----	21	12	27	-----	----	28	5	8	-----	----
15	3	25	-----	----	21	20	6	-----	----	28	12	47	-----	----
15	11	4	-----	----	22	3	46	-----	----	28	20	26	-----	----
15	18	43	-----	----	22	11	25	-----	----	29	4	5	-----	----
16	2	23	-----	----	22	19	4	-----	----	29	11	44	-----	----
16	10	2	-----	----	23	2	43	-----	----	29	19	23	-----	----
16	17	41	-----	----	23	10	22	-----	----	30	3	3	-----	----
17	1	20	-----	----	23	18	1	-----	----	30	10	42	-----	----
17	8	59	-----	----	24	1	40	-----	----	30	18	21	-----	----
17	16	38	-----	----	24	9	19	-----	----	31	2	0	-----	----
18	0	17	-----	----	24	16	58	-----	----	31	9	39	-----	----
18	7	56	-----	----	25	0	37	-----	----	31	17	18	-----	----
18	15	35	-----	----	25	8	16	-----	----	Jan.				
18	23	14	-----	----	25	15	55	-----	----	1	0	57	246·0	21·1
19	6	53	-----	----	25	23	34	247·3	21·4					
19	14	33	-----	----	26	7	13	-----	----					

II. GEOLOGY AND NATURAL HISTORY.

1. *Geological Survey of Pennsylvania.*—The following volumes have recently been issued at Harrisburg:

Report of Progress in Jefferson County (numbered H6), by W. G. PLATT, 216 pp. 8vo, with a colored map of the county.

Third Report of Progress in the Laboratory of the Survey at Harrisburg (numbered M3), by A. S. McCreath. 126 pp. 8vo, with a map.

The Geology of Erie and Crawford Counties (numbered Q4), by I. C. White. 406 pp. 8vo. Includes a paper on the Discovery of the Preglacial Outlet of Lake Erie by J. W. Spencer, Ph.D., with two maps.

The Geology of Blair County (numbered T), by Franklin Platt. 312 pp. 8vo, with Atlas.

The volumes all bear evidence of good work, both in the scientific and practical direction.

Mr. McCreath's Laboratory Report contains numerous analyses of iron ores, coals and cokes, and limestones, with some of fire-clays. Many of the iron ores are from beds of limonite associated with Lower Silurian limestones. The limestone formation No. 2 (or the Calciferous and Chazy), wherever found in Pennsylvania, is stated to have associated with it more or less important deposits of this iron ore, some of them at the bottom, others at the middle, and others at the top beneath the Trenton limestone; and these beds have supplied the larger part of the stock to the furnaces along the Lehigh, Schuylkill and Susquehanna rivers, and the whole of it to the furnaces of Mountain Creek Valley, in Cumberland County, and some others. They occur at intervals in the Cumberland Valley, from the Lehigh River to Maryland, and through Virginia and East Tennessee to Alabama. Other iron ores analyzed were from *Magnetite* mines near Dillsbury, in York Co., connected with the *Mesozoic* sandstone, and still others from Devonian and Carboniferous rocks, and from bogs. The Cumberland Valley ores contain ·018 to 1·787 per cent of phosphorous, but usually under 0·5 ; and they sometimes vary in this respect 0·21 in the same bed.

In the Report on Erie and Crawford Counties Mr. White mentions facts respecting "buried valleys." He states that "the present water-courses meander along the upper surfaces of drift deposits which fill the ancient valleys to various heights above the old rock-beds." About four and a half miles below Meadville, in the valley of French Creek, a boring went down 285 feet through the drift from a level 482 feet above Lake Erie. Conneaut Creek has a drift-filling, according to borings, 180 feet deep. Other similar facts are reported. Conneaut Creek is the only one of the streams that now takes water to Lake Erie. The author refers to similar facts described in the Report of Mr. J. F. Carll, and cites his conclusion that the buried water-ways drained northwestern Pennsylvania toward Lake Erie. Mr. White states, as his own conclusion, that they owe their origin to glacial movement in the opposite direction. Mr. White's Report is occupied mainly with stratigraphical details, but treats also of the disturbances of the region, and of oil-wells and other points of general interest. The oil or petroleum is attributed to generation *in situ* from seaweeds, as urged by Lesquereux. He mentions the occurrence of a grit saturated with oil, in all parts of which were frag-

ments of trees, "like a fallen forest, or rather like a matted natural river-raft." A thin film of coal occurs on some specimens, "but in most cases the wood looks as if it had been converted into petroleum." In the underlying Venango Lower Sandstone and the Chemung flagstones no trace of oil was found, and "the horizontality, the absence of faults, slides, fissures or crushes of any kind, make the ascent of petroleum in the shape of gas a physical impossibility." The paper in this report by Dr. Spencer, on the preglacial outlet of the Lake Erie Basin (into Lake Ontario), has been noticed in this volume on p. 151. Professor Lesley accepts of the general conclusion, but with reference to the suggested origin of the lake-basins by the eroding action of a great ancient St. Lawrence River, he makes the modifying statement that the lake basins "although they may have been traversed by a great river were not properly excavated by it," but by the general abrading action of rills and streams from the rains descending the slopes into it, and probably by the removal of subjacent limestone beds by undermining erosion. To make the drainage system through the Great Lakes complete, so that the excavation by river action could be carried through to the sea, it is necessary to find an outlet for Lake Ontario cut down over 600 feet below the channel of the St. Lawrence, for the lake is over 700 feet deep; and on this point no facts or satisfactory suggestions are given.

2. *First Annual Report of the U. S. Geological Survey;* by CLARENCE KING, Director. 79 pp. roy. 8vo. Washington, 1880. —This volume (recently issued) contains, in reports from Mr. King and the several members of the Geological Survey, a brief review of the work done during the year ending June 30, 1880. The facts stated in these summaries are a promise of a very valuable series of reports on the several regions investigated; and the assurance is given on page 69 of the speedy completion of twelve volumes, as follows: *Geology and Mining Industry of Leadville*, by S. F. EMMONS; *Geology of Eureka Mining District, Nevada*, by A. HAGUE; *The Copper rocks of Lake Superior and their continuation through Minnesota*, by R. D. IRVING; *History of the Comstock mines*, by ELIOT LORD; *the Comstock Lode*, by G. F. BECKER; *Mechanical Appliances used in Mining and Milling on the Comstock Lode*, by W. R. ECKART; *Coal of the United States*, by R. PUMPELLY; *Iron in the United States*, by R. PUMPELLY; *the Precious Metals*, by CLARENCE KING; *Uinkaret Plateau*, by C. E. DUTTON; *Lake Bonneville*, by G. K. GILBERT; *Dinocerata*, by Professor O. C. MARSH.

3. *The Kames of Maine;* by G. H. STONE. 40 pp. 8vo. From the Proceedings of the Boston Society of Natural History, xx, 430–469.—The author describes "kames" as observed by him over a large part of the State of Maine, and on a map gives their positions. They include "kame ridges, and also terrace-like kame-plains." The kames sometimes follow valleys; "freely cross low transverse hills;" are seldom "deflected by hills less

than 100 feet high;" in "no instance cross any hill where, coming from the north, one would have to rise more than about 200 feet in crossing it; in fact the courses of the kames are curiously arbitrary." His theory of their origin is essentially that of Mr. Upham, cited on page 456.

4. *Geology of Staten Island.*—Mr. N. L. Britton has an article, in the School of Mines Quarterly (New York) for May last, on the geology of Staten Island—the large island lying to the south-southwest of New York Island. The geological map accompanying it represents the serpentine area as running nearly through the island, from New Brighton and Stapleton on the north (or rather from Constable Point just north of the island); gneiss as lying against this area on the east; Triassic sandstone and trap on the west; Cretaceous beds on the eastern and southern sides. A geological section is given; but as the gneiss outcrops only near Stapleton, and no strike or dip was taken, it is almost wholly ideal, and, considering the facts on New York Island, its details are very improbable. The asbestos exported from the island—which is only fibrous serpentine and contains therefore 12 to 14 per cent of water—comes from the area near Tompkinsville Landing.

Along with the serpentine or "steatitic rocks, occur superficial deposits of *limonite*, which have resulted from the decomposition of the rocks, in place." It is stated that the amount hitherto mined may be as great as 250,000 tons, while that now annually mined is about 20,000 tons. The limonite of the serpentine area of Rye, New York, (this Journal, II, xx, 32, 1880), is another example of the ore made from the iron minerals of a serpentine region; but at Rye there is some ferriferous dolomite with the serpentine, while the occurrence of disseminated limestone or dolomite on Staten Island is not mentioned.

5. *Apuan Alps.*—A paper on the geology of the Apuan Alps, by B. Lotti and D. Zaccagna, is contained in the R. Comitato Geologico d' Italia, Bulletin Nos. 1 and 2, 1881. The rocks below the lias, are stated to include, beginning below—

a. The central schists: mica schist, talc schist, gneissic and argillaceous schist, with lenticular masses of calciferous schist containing *Orthoceras.*

b. The zone of the Grezzoni: the rock so called being a rough-looking impure limestone sparingly fossiliferous, subcrystalline or ceroid and brecciform; afforded De Stefani a fossil undoubtedly Triassic, *Turbo solitarius;* about five hundred meters on an average in thickness.

c. The zone of the marbles: saccharoidal limestone and dolomite, about 1,000 meters; some traces of *Crinoids* and *Chemnitzia.*

d. The zone of the superior schists: consisting of an alternation of schists, Cipolin marbles, calciferous, micaceous and arenaceous schists, with beds affording *Pentacrinus* and small ammonites of the genera *Phylloceras* and *Ægoceras;* 200 to 1,000 meters in thickness.

The paper gives detailed descriptions with sections.

6. *Jelly-like carbonaceous mineral resembling dopplerite, from a peat bed in Scranton, Pennsylvania.*—An article by Mr. T. Cooper in the number of the Engineering and Mining Journal for Aug. 13, contains the following interesting facts: The remarkable material was discovered in excavating for the new court-house of Scranton. This building-site is in the heart of the town, upon a square which formerly was a swamp, but some years ago was filled with cinder from the iron-works. On excavating for the court-house foundations, the cinder, which was five or six feet deep, was first removed. After this, came a bed of excellent peat, varying in depth from eight to twelve feet. Below the peat, a stratum of muck separated the peat from the hard-pan below. In the muck were veins of the tough black jelly, resembling coal in aspect, except its gelatinous character. When dried slowly it solidifies into a hard, brittle substance, which would be considered by an ordinary observer real anthracite coal. After hardening it does not again soften in water, hot or cold. It burns at a red heat, and leaves an ash resembling the red ash of some coals. It flames on first ignition. The jelly is acted on by alkaline solutions.

A letter to the editors, from Mr. H. Wright, secretary of the Wyoming Historical and Geological Society, dated Wilkesbarre, Aug. 27, 1881, states that an analysis made by the State Chemist afforded

Water, at 212° F.,	66·758
Volatile matter,	9 826
Fixed carbon,	4·012
Ash,	19·404
	100·000

7. *Emeralds from Alexander County, North Carolina.*—Mr. W. E. Hidden, whose important mineralogical labors in North Carolina have been previously mentioned in this Journal (xx, 150; xxi, 128, 159, 160; xxii, 21, 179), has recently announced the discovery by him of emeralds sixteen miles northwest of Statesville in Alexander County, North Carolina. The occurrence of beryls of unusual beauty and crystallographic interest was made known some years since by Mr. J. Adlai Stephenson. Mr. Hidden was led by this fact to make thorough and systematic search in the hope of finding them in place, and he has succeeded in finding not only the ordinary beryls but also true emeralds. The prevailing rock of the region is a feldspathic gneiss with a strike N.N.W., and nearly vertical dip. The surface soil often contains crystals of quartz, rutile, tourmaline, spodumene, beryl, etc., and in cross-fractures in the rock beneath, the minerals have been found by Mr. Hidden in place; of these minerals the emerald-green spodumene (*hiddenite*), and the true emeralds have been the special objects of search because of their value as gems. The first pocket found has been worked to a depth of thirty-three feet and has yielded largely of spodumene, but sparingly of the emeralds; twelve similar cavities have been found within an area of

forty feet square yielding emeralds, while still others have afforded quartz, rutile, monazite, mica and other species. So far as the explorations have been carried, the pockets have been in a crumbling condition and the crystals have been found detached, lying in the bottom of the cavities. As the work is carried down deeper it is to be expected that the rock will increase in firmness. The largest cavity yet discovered had a depth of sixteen feet, and was three feet wide and seven in length. The surface walls were thickly studded with large crystals of quartz, some of twenty-five pounds in weight, and with them nine fine emeralds. Their form was that of a twelve-sided prism (*I* and *i*-2), with basal planes, all well polished. The largest crystal had a length of eight and one-half inches and an average diameter of one inch. The others varied in length from two to six inches. Most of the crystals found are vertically deeply striated or ribbed, and are transparent, though not free from flaws. In some of the crystals the color near the surface is the deepest and the core is nearly colorless. The North Carolina emeralds do not quite equal in color those from Muso, New Granada, but are nevertheless very beautiful and will bear comparison with those from other known localities.

8. *Brief notices of some recently described minerals.* (Continued from page 155.) ILESITE.—A white friable mineral with a bitter, astringent taste, readily soluble in cold water. An analysis afforded Dr. Iles—SO_3 35·85, MnO 23·18, FeO 4·55, ZnO 5·63 H_2O 30·18=99·39, corresponding approximately to Mn(Fe, Zn) SO_4+4aq. Occurs with pyrite and sphalerite forming a band two to eight inches in width; locality, Hall Valley, Park Co., Colorado. Named after Dr. M. W. Iles, of Leadville.—*Mining Index*, Leadville, Nov. 5, 1881.

SEMSEYITE.—Briefly mentioned by Kreuner as a mineral containing lead, antimony and sulphur, occurring in gray crystals, and resembling plagionite. Found with diaphorite, sphalerite and pyrite at Felsöbanya.—*Ungarische Revue*, April, 1881.

ÅNNERÖDITE.—Occurs in crystals closely related to columbite both in habit and angles. H.=6. G.=5·7. Luster metallic to submetallic. Color, black to blackish-brown. Translucent in thin splinters. Fracture sub-conchoidal. An analysis by C. W. Blomstrand gave Cb_2O_5 48·13, SnO_2 0·16, SiO_2 2·51, ZrO_2 1·97, ThO_2 2·37, U_2O_3 16·28, Ce_2O_3 2·56, Y_2O_3 7·10, PbO 2·40, FeO 3·38, MnO 0·20, CaO 3·35, MgO 0·15, K_2O 0·16, Na_2O 0·32, Al_2O_3 0·28, H_2O 8·19=99.51. The formula deduced is $R_2Cb_2O_7+2\frac{1}{2}$ aq, which makes the mineral related in composition to samarskite. Found in a pegmatite vein at Ånnerod, near Moss, Norway. Described by W. C. Brögger.—*Geol. För. i. Stockholm Förhandl.*, v, 354, 1881.

ZINCALLUMINITE.—Found in very small thin hexagonal crystals; optically, uniaxial negative. Color white, or slightly tinted with blue. An analysis by Damour gave SO_3 12·94, Al_2O_3 25·48, ZnO 34·69, CuO 1·85, H_2O 25·04=100. From the

zinc mines at Laurium, Greece, associated with smithsonite, serpierite and several undetermined species. Described by Bertrand and Damour.—*Bull. Soc. Min. de France*, iv, 135, 136, 1881.

ALASKAITE.—Massive, small foliated. G.=6·878. Luster, metallic. Color, whitish lead-gray. Opaque. Analysis (after deducting impurities), S 17·63, Bi 56·97, Sb 0·62, Pb 11·79, Ag 8·74, Cu 3·46, Zn 0·79=100; another analysis gave 3 p. c. Ag, and 5·38 p. c. Cu. The formula deduced is $(R,R)S+Bi_2S_3$. Occurs intimately mixed with quartz, barite, chalcopyrite and tetrahedrite at the Alaska mine, Poughkeepsie Gulch, Colorado. Described by G. A. König.—*Amer. Phil. Soc. Philad.*, 1881, 472.

9. *Artificial formation of the Potash-feldspar, Orthoclase;* by C. FRIEDEL and E. SARASIN (Bull. Soc. Min. de France, iv, 171). —The process used by these chemists for the formation of orthoclase in crystals consisted in heating together in a tube of steel having red copper within, for 15 to 20 hours to a temperature between 400 and 500° C., a mixture one part of aluminum silicate and another of a potassium silicate rich in alkali. A higher temperature was disadvantageous, it producing a crystallization of the silica either as quartz or as tridymite. The trials gave a crystalline powder, which was made up of crystals of orthoclase large enough to be studied crystallographically. Thoulet's method gave for the specific gravity that of orthoclase. An analysis afforded alumina 15·59, potash 14·38, leaving for the silica 70·03. There is here an excess of silica of 6·30 per cent, which was due to the presence of some free silica; the other ingredients have the orthoclase proportions. The authors did not succeed when the mixture was made to consist of silica, alumina and potash, in the proportions they have in orthoclase.

10. *English Plant-Names from the Tenth to the Fifteenth Century.* By JOHN EARLE, M.A., Rector of Swanswick, Professor of Anglo-Saxon in University of Oxford. Oxford: Clarendon Press, 1880. 16mo, pp. cxii and 122.—A notable little book, consisting in the first place,—yet in the volume occupying the last place,—of sundry Saxon vocabularies in which "the native plant-names have been preserved in the most primitive form extant, printed for the use of friends of Saxon studies" without any idea of making a book. To this is prefixed an Introduction, on the history of plant-names from Theophrastus down to the modern system of nomenclature; the signification of the old native plant-names; their relation to the Roman ones; grammatical elements of English plant-names; on the neglect of vernacular names, etc. Of the matters linguistic we are not now to speak; and probably Professor Earle is only a superficial botanist. But his sketch of the history of nomenclature, and of the development of mere herb-lore or the rude knowledge of simples into botanical science is as critically excellent as it is terse and fresh. Indeed, we know of nothing half so good within so small a compass. Then we begin to understand "the fascination of vernacular plant-names," which, as the author remarks, "has its foundation in two instincts,

the love of nature and curiosity about language. Plant-names are often of the highest antiquity and more or less common to the whole stream of related nations. Could we penetrate to the original suggestive idea that called forth the name, it would bring valuable information about the first openings of the human mind towards Nature; and the merest dream of such a discovery invests with a strange charm the words that could tell, if we could understand, so much of the forgotten infancy of the human race."

Here is a good word for the amiable science, considered educationally. "Historically almost the first of sciences, Botany is naturally and eductionally [educationally?] first in order to the enquiring mind. Its objects are near our homes, awakening to our minds, and inviting to our touch. Botany is adapted to be the universal preparatory science, the science to infuse the scientific sense."

While giving a series of examples of the changing meanings of a certain class of words, the author goes singularly astray in a single instance: e. g. "In England *farmer* means an occupier, in America it means a hired labourer." No, indeed: it means a cultivator of the land who is *not* a hired laborer: he is commonly the owner of his *farm* in fee simple. A. G.

11. *Familien Podostemaceæ.* Studier af Dr. EUG. WARMING. 1ste Afhandlung.—This is a paper in the Memoirs of the Royal Academy of Sciences of Copenhagen, being the commencement or first part of an extended treatise on the *Podostemaceæ*, morphological, anatomical, and systematic. This singular family of Phænogamous plants, simulating *Algæ* in vegetation, takes its name from our *Podostemon ceratophyllus*, of Michaux's Flora, the only North American representative, and the only one inhabiting the North temperate zone. Having been well supplied by Mr. Canby with a stock of plants in spirit, in all stages of growth, Dr. Warming has taken this species for particular study, and his anatomical and morphological investigation of its organs of vegetation is here presented. The body of the article is in the Danish language. But an abstract and also the full explanation of the plates are in French. The whole fills 34 quarto pages and is illustrated by six plates, crowded with figures, drawn and lithographed by the author himself. Three of the six plates and half of the fourth are devoted to our *Podostemon*. A. G.

12. *Recherches sur la physiologie et la morphologie des ferments alcooliques.* By EMIL CHR. HANSEN.—The present paper, extracted from the proceedings of the physiological laboratory of Carlsberg, Copenhagen, for 1881, treats of *Saccharomyces apiculatus* and its occurrence in nature. This ferment, according to Hansen, is found during the warm season on juicy fruits, as gooseberries, cherries, plums, etc., and is carried to the earth by winds and rain and passes the winter buried in the soil. In fermentation it acts as a bottom yeast but possesses only a feeble action, since, while the common yeast, *Sacc. cerevisiæ* produces six volumes of alcohol, *Sacc. apiculatus*, produces only one. The beer

which it produces has a peculiar taste and odor. The species produces no invertine, nor can it cause an alcoholic fermentation in saccharose solutions. The cells are very tenacious of life, can be kept dried several months, and in this condition exposed to marked variations of the thermometer without apparent injury.

W. G. F.

13. *On an Organism which penetrates and excavates Siliceous Sponge-spicules* (Spongiophagus Carteri); by Professor P. Martin Duncan.—In a communication which I made to the Royal Microscopical Society on June 8, 1881, the presence of green-colored cells on siliceous sponge-spicula, in relation to minute penetrations into their axial canals, was asserted. The occurrence of a granular plasma of the same tint within enlargements of the axial canals was noticed; and the penetration and erosion were stated to be due to the organism. The cells which were observed within hollows on the surface of a spicule, and also on perfect spicules in positions where erosion from without inwards could readily occur, were very small,—not more than $\frac{1}{7000}$ inch in length, and very much less in height. Their dimensions, however, corresponded to those of certain circular patches with hollowed-out bases, which are the first stages of the penetration through the spicule down to the axial canal. The penetration of the spicule down to the central canal is followed by the growth of the organism, which appears to erode the silica and enlarges the canal in a most remarkable manner.

After a while the spicule suffers solution of its continuity by the thinning from within, and the thinnest flakes present a granulated appearance.

Since writing that communication I have observed siliceous sponge-spicules, obtained from great depths, which are affected by an organism whose cells are much larger and whose penetrations therefore are wider and much more visible. On the head of a large spinulate spicule I found many circular pits, each containing an organic mass without definite cell-wall, and yet granular and green in color by transmitted light. These pits are shallow and are $\frac{1}{2000}$ inch in diameter. Similar pits and of the same dimensions are seen on other spicules; but they are deep and resemble cylindrical tubes with hollowed-out bottoms. Some reach the axial canal, which has become enlarged. The penetrations contain granular organic substance; and so do the enlarged axial canals. The walls of the enlarged axial canals are frequently very irregularly eroded and look "worm-eaten;" the hollows are, moreover, green with the very visible granular matter.

Thus there are two dimensions of the penetrations. The first kind of cell found on the spicules resembles somewhat the simple zoospores of *Achlya penetrans* Duncan (Proc. Royal Soc., vol xxv, pl. vi); the second is larger; and in both there is a decided green tint. No ramifications of the penetrating cylindrical tube occur; and it pierces perpendicularly to the surface of the spicule, or, it may be, slightly aslant.

The presence of pits on the surface of sponge-spicules was noticed by Kölliker as a peculiar degeneration of the structure. Dr. Carter described and figured pits in the outer part of a spicule, and distinctly referred them to the action of a vegetable cell, in the Ann. & Mag. Nat. Hist. ser. 4, vol. xii, p. 457, pl. xvi, figs. 8, 9. None of the pits seen by my friend reaches the axial canal; but some of them terminate in globular excavations.

It is evident that the assimilation of the organic substance in the sponge-spicule by the vegetable organism produces the destruction of the siliceous structure; and probably the colloid silica unites with the protoplasm of the destroyer and forms an organic compound with it.

Large cells and small nucleus-like cells operate, producing penetrations of corresponding diameters through the spicule down to the axial canal. The vegetable growth occurs there; and the amount of erosion does not appear to be in relation with the size of the primary penetration.

The organism is not an *Achlya;* and all that can be said is that it consists of cell-like bodies without very definite cell-walls, but evidently with a very delicately limiting texture surrounding a granular greenish plasma, and that there is much free and non-cellular plasma with bodies like small nuclei, the whole having a faint green tint. I have named this very lowly organic substance (which is probably a plant) *Spongiophagus Carteri.—Ann. & Mag. Nat. Hist.*, Aug., 1881, p. 120.

14. *Bulletin of the Museum of Comparative Zoology at Harvard College.* Vol. VI, Part ii, No. 12. E. L. Mark on the *Maturation, Fecundation and Segmentation of Limax campestris* Binney. pp. 173–625, 8vo, with 5 double plates. — A profound microscopic research throwing new light on the metamorphosis of the nucleus and other points in the earliest stages of egg-development, reviewing at length, with criticisms, previous researches on the subject, and giving an extended bibliography.

15. *The Palæocrinoidea.*—Part II of Wachsmuth and Springer's revision of the Palæocrinoidea is contained in the Proceedings of the Academy of Natural Sciences of Philadelphia for 1881, commencing with page 177. It is devoted to the Family Sphæroidocrinidæ, under which are included the Sub-families Platycrinidæ, Rhodocrinidæ and Actinocrinidæ. It is a long and very valuable paper.

Cosmos les Mondes: Revue hebdomadaire des Sciences et de l'Industrie, fondée et dirigée pars M. l'Abbé F. Moigno, Paris. —The valuable weekly review, *Les Mondes,* commenced by M. l'Abbé Moigno in 1852, appears now in new form, enlarged in size and improved in appearance. The Abbé still retains the direction of the review, but he has the assistance of a group of collaborators, under whose combined efforts it promises to have an increased sphere of usefulness in the future.

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